A

TREATISE,

ON THE

FEVERS OF JAMAICA, &c.

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TREATISE

ON THE

FEVERS OF JAMAICA,

WITH SOME

OBSERVATIONS

ON THE

Intermitting Fever of America,

AND AN

APPENDIX,

CONTAINING SOME HINTS ON THE MEANS OF PRESERVING THE HEALTH OF SOLDIERS IN HOT CLIMATES.

By ROBERT JACKSON, M.D.

Ne mea dona tibi studio disposta fideli,
Intellecta prius quam sint, contempta relinquas. Lucret. lib. 1.

—διατειρα τοι
Βροτων ελεγχος. Pind: Olym. 4.

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PREFACE.

THE obſervations contained in the following pages, were made during the time that I lived in Jamaica, or while I attended ſome part of the army in America. The materials were collected between the years 1774 and 1782; and the preſent performance would have been offered to the public before this time, had I ſooner found leiſure to attend to the buſineſs of publication. The inſufficiency of Dr. Hillary's work, the moſt eſteemed book on the diſeaſes of the Weſt Indies, and the only one with

which I was acquainted while I remained in that country, furniſhed me with a motive for the undertaking; a motive, which may be thought, perhaps, no longer to exiſt, as two treatiſes have been publiſhed lately by Dr. Hunter and Dr. Moſeley, expreſsly on the diſeaſes of which I have written. I might remark, that the preſent attempt, ſuch as it is, was nearly completed before the treatiſes to which I allude came to my hands. The views which they afford of fevers, as they differ from each other, ſo they likewiſe differ from thoſe which I have ventured to advance. I have weighed their merits maturely, and cannot ſay that I diſcover any information, which gives me cauſe to change thoſe opinions which I had formed, or which renders the publication of the preſent work

unneceſſary. I may obſerve, that Dr. Hunter details, with candour and perſpicuity, the mode of practice, which was followed by the moſt reſpectable medical people of Jamaica, at the time that I lived in the iſland. He perhaps preſcribes the bark in larger quantities, than was then cuſtomary; but I do not perceive any thing in the plan of treatment eſſentially new; neither will Dr. Moſeley, though he purſues innovation with great eagerneſs, be better able to eſtabliſh his claim to original diſcoveries. The plentiful and long continued purging, on which he places a conſiderable ſhare of his merit, has been a favourite practice with numbers for many years paſt; and the free uſe of the lancet, which he recommends ſo much in fevers, was employed in ſeveral diſtricts of

Jamaica, before this author's name was known. Dr. Spence, a practitioner of ſome eminence at Lucea, in the Weſtern extremity of the iſland, wrote a pamphlet (I believe in the year 1776) with a view to enforce its ſafety and utility, in promoting the cure of the general claſs of febrile diſeaſes. The publication was well received, and ſerved to remove the dread of the lancet, which ſome people till then had falſely entertained.

I have thus explained the motive which induced me to write; and I leave it to people of experience to judge of the manner in which I have executed the taſk. I ſhall only obſerve, that, when I firſt intended to prepare my obſervations for the inſpection of the public, I had no other deſign than to mention

fuch circumſtances in the hiſtory and cure of fevers, as did not appear to be generally known. Having ſpent the earlier part of my life in ſituations, which did not admit of a continued plan of ſtudy, I had not till lately much acquaintance with the opinions of medical writers. About four years ago, when I found a ſettled abode, I began occaſionally to look into the works of the ancient phyſicians. In examining Hippocrates, I diſcovered ſuch a ſimilarity in the fevers of the Archipelago, with thoſe of Jamaica, that I ſuſpended my deſign of publiſhing, till I ſhould have given that author's writings a careful peruſal. I ſoon was convinced that many obſervations, which I had conſidered as my own, were actually known to this father of phyſic; and though I

was probably disappointed in being anticipated in offering something new to the public, I was still gratified by the coincidence of remark, that gave me a confidence in my accuracy which otherwise I durst not have assumed. After I had carefully perused the writings of Hippocrates, I consulted and compared such other of the Greek physicians, as I was able to procure. I even descended with a similar examination to the present times; but as my collection of medical writers, particularly of modern ones, is small, I have probably omitted some who ought to have been mentioned, or perhaps advanced observations as my own, which in reality belong to others. If I have done so, I must be allowed to say, that I have done it without conscioufness.

It may appear, perhaps, that I have treated the opinions of great names with too little reſpect; but if facts have at any time occurred to me, which contradict eſtabliſhed theories, I ſhould hope that theſe facts will be examined before they are rejected. No medical authority ought to prevail over the certain evidence of truth. I am not conſcious of having miſrepreſented, for the ſake of a theory, the minuteſt circumſtance of what I have actually ſeen;—if I have been miſtaken in any inſtance in forming concluſions, I ſhall receive the correction of my inaccuracies with gratitude.

CHAP. I.

OF THE GENERAL CHARACTER OF THE REMITTING FEVER OF JAMAICA.

AS it is perfectly well known, that fevers, which are eſſentially in themſelves the ſame diſeaſe, vary in their appearances, from difference of climate and ſeaſon, it would be very ſuperfluous to attempt any proof of what is ſo generally acknowledged. Even Hippocrates, who lived more than two thouſand years ago, does not ſeem to have been unacquainted with this fact. This induſtrious phyſician, as we learn from many paſſages in his works, not only viſited the various iſlands in the Ægean Sea; but travelled likewiſe into various parts of the adjacent continents. The motive of his journeys, we are taught to believe, was principally to obſerve the different ſituation of places, and to mark their correſponding diſeaſes. He has deſcribed very fully, in a curious and uſeful treatiſe, the effects of air and local ſituation on the human frame; and in the hiſtory of fevers, related in the books of Epidemics, has ſeldom forgotten to mention, not only the general conſtitution of the ſeaſons with reſpect to diſeaſes; but likewiſe the nature of the climate, where his obſervations were more particularly made. The ſpecies of fevers, it is true, that are found in the writings of Hippocrates are extremely multiplied; yet this author ſeems ſtill to have been clearly of opinion, that diſeaſes, which are eſſentially the ſame, aſſume, in ſome reſpects, a different appearance in the iſland of Thaſus, and at Abdera, on the contiguous

coaſt of Thrace. Obſervations to the ſame effect have been made by many others in different parts of the world; nor is the fact capable of being better illuſtrated in any country than in Jamaica; where a very ſmall change of place, or the ordinary revolution of ſeaſons, are often obſerved to influence in a high degree, the appearances of the common endemic of that iſland. But this obſervation,—that local ſituation and the change of ſeaſons, are the cauſe of varieties in the appearances of the ſame diſeaſe, has been ſo long known, that it is almoſt unneceſſary to mention it; nor would it have been repeated now; unleſs to obviate any objections which might be made to the hiſtory, that is given in the following pages, ſhould it not be found exactly to correſpond, with that which has been obſerved by others, who have lived in the other iſlands of the Weſt Indies, or in other parts of Jamaica; or perhaps even in the ſame part of Jamaica, at a different period of time. It is preſumed, indeed, that the ſame fundamental diſtinctions of fever obtain in every iſland within the tropics; yet the deſcription that is given in this place, (it may not be unneceſſary to mention), is affirmed to be ſtrictly exact, only in the diſtrict about Savanna la Mar; and that only for a ſhort ſpace of time: viz. from the year 1774 to the year 1778.

The fever, that chiefly prevailed at Savanna la Mar during the period mentioned above, was uſually mild in its ſymptoms, and more regularly remitting in its form than I underſtood the endemic diſeaſe to be in moſt other parts of the iſland. To what circumſtances in the local ſituation this might be owing, I do not pretend to determine. Savanna la Mar is ſituated cloſe by the ſea: its particular ſcite, and the ſurrounding country to the diſtance of ſeveral miles is perfectly level; whilſt in conſequence of its being open to the ſea on the eaſt, it is viſited early, and conſtantly by a ſalutary and refreſhing breeze. There

is a small rivulet, indeed, which, losing itself almost entirely in mud, forms a morass that partly surrounds it on the north. From vicinity to this morass the situation of Savanna la Mar has been suspected to be unhealthy; but it is not so in fact. The sea at high water, particularly in the spring tides, overflowing the swampy ground, contributes in a great measure, perhaps, to diminish the more usual noxious qualities of the marsh exhalation. The opinion of many eminent writers, however, is not altogether favourable to this idea; but there seems to be reason to doubt, whether the opinion formed by those writers in this instance, is the result of accurate and careful observation; or if it is merely a suggestion of theory. I am inclined to the latter way of thinking, as I have never found the neighbourhood of Salt-marshes, in the different parts of America that I have had the opportunity of visiting, less healthful than the rest of the country:—on the contrary, they were frequently more so.

Before proceeding to a particular description of the fever, that chiefly prevailed in the district of Savanna la Mar, it may not be superfluous to give the outline of its character, that we may be the better enabled to trace its resemblance with the fevers of other climates, and to determine its place in the general class of febrile diseases. The common fever of Savanna la Mar, which, as I mentioned before, was usually mild in its symptoms, and regular in its form, seems to be properly ranked with those, that strictly speaking are called remitting. It is difficult to fix the boundaries between remittents and intermittents, if a fixed boundary actually exists. The paroxysms of the fever of Jamaica are observed, in many instances, to terminate in more perfect remissions than the paroxysms of the endemic of North America, which is known to be fundamentally an intermitting disease. Hence authors generally are of opinion, that all the difference which appears to take place in those fevers, depends

merely on an accidental cauſe: viz. on the greater or leſs heat of the climate. I ſhall not be poſitive that it is not ſo; yet I cannot help remarking that certain appearances incline me to be of opinion, that there ſubſiſts, between the endemic of Jamaica and the endemic of North America, a difference, in ſome degree, fixed and eſſential. I cannot pretend to aſcertain the difference preciſely; or to offer a conjecture about the modification of the morbid cauſe in which it conſiſts; yet the following circumſtances give reaſon to believe, that to a certain degree at leaſt it actually takes place. The common fever of Jamaica, for inſtance, was not only diſpoſed to terminate of its own accord; but it was diſpoſed to terminate on certain critical days, often at an early period, and by ſigns of criſis too clear to be miſtaken; neither did the Peruvian bark, in the manner at leaſt in which it was managed, ever cut ſhort its courſe with certainty. The endemic of America on the contrary often laſted long. It frequently, indeed, changed to another diſeaſe after a length of time; but no period could be aſſigned for its natural termination. The ſigns of criſis, it may likewiſe be remarked were ſo obſcure as ſcarcely to be diſtinguiſhed with the cloſeſt attention; at leaſt for my own part I will own, that after an experience of ſeveral years, and the greateſt care in obſerving the minuteſt circumſtances, I never yet was able to ſay with confidence, that the endemic of America, particularly in the northern provinces, was gone not to return again, till the hour of its return was paſt: neither did the Peruvian bark, though its effects were ſo equivocal in the fever of Jamaica, ſcarcely ever fail of ſtopping the progreſs of this diſeaſe. To which we may add, that the complaint, which ſtrictly ſpeaking is called the intermittent, or ague and fever, can ſcarcely be ſaid to belong to Jamaica; at leaſt it was not known at Savanna la Mar. In the courſe of four years I did not once obſerve it; and thoſe, who had lived much longer at this place, aſſured

me they had never seen an instance of it; unless in persons who were newly arrived from aguish countries. This is a fact of some importance, as it shews to us, that though the proper intermittent is not the endemic disease of the country it is still capable of existing in the climate:—no weak argument, that, the two diseases of which we have been speaking, actually do possess some difference in the modification of the general cause, though we are unable to define the precise bounds and limits of it.

The circumstances which I have mentioned might incline us to be of opinion, that the fever of Jamaica, and the common endemic of America are not exactly the same disease; that is, that though they depend on the same general cause; yet that this cause undergoes some fixed and permanent modification in those different climates, so that an essential difference actually arises. But though this really appears to be the case; yet I must acknowledge, that the reigning epidemic of the southern provinces of America, often loses its distinctive marks of intermission in the hot months of summer; whilst it approaches, in other respects, so near to the fever of Jamaica, as to be distinguished from it with difficulty. This was particularly the case at Ebenezer in Georgia, in the year 1779, during the months of June and July. A cold fit was seldom observed in this place; unless perhaps in the first attack; lowness, languor, head-ach, pain of the back and other disagreeable feelings remained even in the most perfect remissions: the disease was likewise much disposed to terminate of its own accord: on the usual critical days, frequently by signs of crisis, that were far from being obscure: yet though these resemblances were so very striking, the fever of Ebenezer was perfectly under the controul of Peruvian bark, which was not exactly the case with that of Jamaica. I must remark, however, that bark was used with a freedom in America, that I never thought of attempting in the West Indies.

But though it ſtill may be thought doubtful by ſome, whether the autumnal fever of aguiſh countries, and the endemic fever of Jamaica are characteriſtically different, or eſſentially the ſame diſeaſe; yet it is not ſo difficult to trace a ſpecific reſemblance, between this fever of the Weſt Indies, and the prevailing endemic of the Ægean Sea. The fever deſcribed in the Epidemics of Hippocrates has every ſtriking feature of the diſeaſe which is the ſubject of the following treatiſe. The general character is the ſame; the courſe and mode of termination are often alike. If the duration is ſometimes different, it is probably much owing to the more deciſive practice of the moderns: for though it remains uncertain, whether a remedy has yet been diſcovered, which abſolutely cuts ſhort the fever of Jamaica in the midſt of its courſe; yet no doubt remains, that there are various modes of treatment, which may, and actually do render the ordinary changes of the critical day, decidedly critical. The fevers of the Ægean Sea, as deſcribed by Hippocrates, and of Minorca, as deſcribed by the accurate Cleghorn, bear the neareſt reſemblance to the endemic of Savanna la Mar. The fevers of Italy, of different parts of the continent of Aſia, as deſcribed by various writers, as well as the fever of America, of which I have had perſonal experience, however obſcure their remiſſions, ſeem rather to be degenerated intermittents, than the diſeaſe deſcribed in the following pages. But that I may not be thought to inſiſt too much on this opinion, which few, perhaps, may conſider as of much conſequence, I ſhall relate the hiſtory of the fever of Savanna la Mar, as accurately as I can, leaving it to others to determine, whether it ſhould be conſidered as an intermittent degenerated into a remitting form, in conſequence of the greater heat of the climate, or allowed to poſſeſs ſomething characteriſtic in its own nature.

CHAP. II.

OF THE DIFFERENT TYPES OF PERIODICAL FEVERS.

A Knowledge of the types and periods of fevers, though ſo neceſſary to be well underſtood by thoſe who pretend to cure the diſeaſe, has unfortunately been little attended to by the practitioners of Jamaica. In that country, I met with ſome who were able to foretel the returns of the ſingle tertian; but I met with none, and I have good reaſon to believe that there actually were few, who troubled themſelves about forms of greater complication. There were many, on the contrary, who, blinded by theories of accumulated bile, ridiculed altogether the idea of this ſtated regularity in the movements of nature. To enter into a diſpute with ſuch would be labour loſt. The exiſtence of a regular type in the fevers of the Weſt Indies does not admit of a doubt, yet we muſt not forget to mention, that thoſe types are traced with greater difficulty in that country, where remiſſions are obſcure, than in others, where every paroxyſm is uſhered in by a cold fit. In Jamaica it is impoſſible to follow them without the moſt careful attention, or perhaps to attain a clear and ſyſtematic knowledge of them, without writing down and analyzing many of thoſe caſes which occur in practice. I remember to have been impreſſed with the idea, at a very early period, that one obſervation made by myſelf would in reality be more uſeful than twenty equally important in themſelves, which I only retained in my memory from reading. Convinced of this truth I ceaſed to look for information in books, from the time that I arrived in the Weſt Indies; but, truſting wholly to

my own experience, wrote down minutely, in the presence of the sick, the history and cure of the most important cases of fever that occured to me. At stated times I reviewed that which I had done, and arranged under proper heads the most striking circumstances, that I found recorded in my notes. Among other unexpected appearances I observed a regularity and order in the types of fevers, of which at first I had no idea. But though the laws of nature appeared to be fixed and stable, in producing this varied but stated regularity of form; yet a knowledge of those laws was not easily attained. Two years were spent, and not fewer than a hundred cases were analyzed, before my views of the subject were in any degree accurate. The labour, perhaps, was superfluous; there being many authors who have described minutely every variety and every combination of type that has ever been observed to take place. But as I had little knowledge of the writings of others at this period, I shall content myself in the first place with relating the history of types as they occurred to my own observation; at the same time that I shall not omit to take notice occasionally of the more constant peculiarities, which are found in authors of credit, who have practised in different countries. The influence of climate I may observe is of considerable effect in modifying the various forms.

The single tertian, the period of which is forty-eight hours, is a form of fever that occurred frequently in Jamaica, particularly in the dry and healthy season. Its course was easily traced, as the remissions were often distinct, and the accessions sometimes distinguished by a slight horror or shivering.

But though the above-mentioned type was by no means of rare occurrence at Savanna la Mar; yet the double tertian, with similar paroxysms on alternate days, was still more common, particularly in the rainy and sickly months. This form of fever, which seemed to consist of two single tertians, that ran a separate

and independent courſe, began uſually in the morning. Its hour of invaſion was from eight to ten; and its acceſſion was generally diſtinguiſhed by a cold fit. The paroxyſm, which for the moſt part was regularly formed, declined after a continuance of eight or ten hours; and the patient remained free from fever, not only during the night, but during the following day, till four in the afternoon, or later. A feveriſh indiſpoſition uſually came on then, which continued the whole or the greateſt part of the night. A remiſſion took place; but it was ſoon ſucceeded by a paroxyſm ſimilar in its ſymptoms, and manner of attack, to the paroxyſm of the firſt day. This having declined towards evening, the patient, as formerly, was free from fever during the night and the day following. At the uſual hour, however, or more generally before it, a paroxyſm more diſtinctly formed in all its parts, and more violent in degree, than the preceding one which correſponded with it, returned, and continued till morning. It ſubſided; and was ſoon ſucceeded by the paroxyſm of the fifth day, which declined, as formerly, after the uſual duration. Such was the general courſe and progreſs of the diſeaſe.—The fever of the odd day, which began for the moſt part in the morning, uſually returned later and later, and with decreaſing violence every ſucceeding paroxyſm; whilſt the fever of the even day, which generally began in the afternoon or evening, as uſually returned earlier, and when this was the caſe, frequently encreaſed in force. The fever which came on in the morning generally began the diſeaſe. It was for the moſt part a fever of complete and regular paroxyſms from the firſt attack. The fever of the evening, on the contrary, was little more than an indiſpoſition in its beginning; nor was its time of appearing at all certain. It often was not perceived till the evening of the fourth day; ſometimes not till later, neither was its continuance of a fixed duration. Sometimes it went on after the fever

of the morning had ceased; and in other cases it terminated, while the other pursued an uninterrupted course.

The type, which was so frequent in Thasus, and the other islands of the Ægean Sea, seems to be a species of the double tertian. Mention is made frequently of such a form in the Epidemics of Hippocrates; yet the double tertian of Hippocrates is directly opposite, in some respects, to the type which I have just now described.—The evening fever generally began the disease in those islands: hence the great exacerbations, and consequently the crisis were frequently on the even days. Such also appears to have been sometimes the case in Minorca: yet Cleghorn acknowledges, that a type, similar to that which I have described as prevailing so commonly at Savanna la Mar, was by much the most frequent form of the abovementioned island. It was also, I may add, frequent in the southern provinces of North America, particularly in the hot months of summer and autumn.

A quotidian type has been described by almost every author, who has written on the subject of intermitting and remitting fevers: neither can it be denied, that forms of disease frequently occur, where the paroxysm returns every day at the same hour, with symptoms so nearly alike, that an ordinary observer can perceive no difference. Yet Mr. Senac, an author of considerable eminence, boldly maintains, that a real quotidian type does not exist in nature. The question, perhaps, is not easily determined. I can only mention from my own experience, that I have frequently seen fevers with quotidian exacerbations of such a kind, that there was no perceivable difference between them, either in symptoms or in manner of attack; yet I must likewise own, that these exacerbations were generally in the evenings; and that the disease was not in every respect of a distinct intermitting form.

I have now mentioned those types that are most

frequently met with in the fevers of Jamaica; yet beſides theſe, there now and then occur others of more complicated and perplexing forms. Thus, I have ſometimes ſeen at Savanna la Mar, a fever, which ſeemed to be compounded of a ſingle tertian with a quotidian. In the ſpace of forty-eight hours there were three ſeparate exacerbations, two of which were exactly alike, that it was impoſſible to perceive a difference; whilſt the whole three followed each other in ſuch an order of time, that to a ſuperficial obſerver, there appeared to be only one long paroxyſm of thirty-ſix hours or more. An example will make it more plain. On Monday, for inſtance, the paroxyſm of a fever was obſerved to begin about nine in the morning, preceded by ſome degree of coldneſs and ſhivering. This paroxyſm was uſually violent in its firſt attack: But declined gradually towards evening; though before it was gone off totally, another exacerbation commenced, which continued the whole of the night. This likewiſe abated on Tueſday morning; and the patient remained apparently free from fever till five in the afternoon or later. A paroxyſm then made its appearance, ſimilar to the paroxyſm of the preceding evening. It ran over a courſe of ſimilar duration, and ſcarcely had abated on Wedneſday morning, when a paroxyſm reſembling that of Monday ſucceeded it; which, as formerly, declining towards evening, was ſoon followed by an exacerbation, that laſted till the morning of Thurſday. On Thurſday, as on Tueſday, there was no fever till late in the afternoon; when the evening exacerbation returning at the uſual hour, proceeded in its uſual courſe.

The tertian type, ſimple, or variouſly compounded, is the form of fever, which prevails moſt univerſally in all climates. The quartan in thoſe countries were I have lived was rare; and the exiſtence of a real quotidian, perhaps, is doubtful. To that compound form,

which I have juſt now deſcribed, I ſhould be inclined to give the name of Semitertian. It is expreſsly the diſeaſe, which I now find has been deſcribed under this name by Hoffman; but it is more difficult to determine exactly, if it is the Hemitritæus of the ancients. The Hemitritæan form is frequently mentioned by Hippocrates; but his definition is too looſe to enable us to judge preciſely of its nature. It is in fact impoſſible to ſay with certainty, whether it is to a type ſimilar to that which I have deſcribed, or to the extended and ſubintrant paroxyſms of the double tertian, that he has applied the name. This laſt, indeed, ſeems to have been the idea of many of the ancients, particularly of Celſus and Agathinus.—The phyſicians of the earlier ages, were leſs curious in minute diſtinctions than their followers; and probably applied the name of Hemitritæus to thoſe fevers, the paroxyſms of which were ſimply of unuſual duration, no leſs than to thoſe that were of a complicated or compound nature. This actually appears to have been the caſe for a great length of time; but at laſt, and not long indeed before the days of Galen, the ſophiſtical genius of the profeſſors of medicine, which exerted itſelf chiefly in things of little importance, multiplied the types of fevers to an endleſs variety, and attempted to eſtabliſh diſtinctions which have no exiſtence in reality. Galen, who is copious in moſt things, has diſcuſſed very fully the ſubject of types in general, and deſcribed particularly the nature of the Semitertian at great length. But whatever credit may be otherwiſe due to the obſervations of this illuſtrious writer, it is evident that the deſcription, in the preſent inſtance, is merely the reſult of theory. He has attempted, indeed, to illuſtrate his opinion by an example; but the caſe he has furniſhed us with, is conſtantly varying its hour of attack; and if accurately examined, appears rather to be a triple tertian, properly ſo called, than the form of fever that

I have described above. After the Greeks we may next take a short view of the Arabian physicians, who, as they borrowed much of their knowledge from the writings of Galen, likewise adopted his idea of the compound nature of the Semitertian. Avicenna, the most eminent among them, has defined this form of disease with a good deal of precision; but we do not find that he has added any thing very material to the opinions of his predecessors. Galen indeed, had discussed the subject so fully, that, though succeeding writers sometimes changed names, they do not seem in reality to have furnished much new observation. As we descend to less remote times, Hoffman and Cleghorn are the most accurate of the moderns, on this subject, who have yet come to my hands. The former describes under the name of Semitertian the express form of disease, that I met with in Jamaica; the latter refers this title to the extended and subintrant paroxysms of the double tertian. It is with unwillingness that I differ in opinion from Cleghorn, who has thrown more light on the history of periodical fevers, than perhaps, all the writers of his time: yet I cannot help observing, that I never recollect to have met with an original Semitertian, such as that he has described in his well-known treatise. I must own, indeed, that I have several times seen the morning fever of the double tertian anticipate, so as to be mixed with the decline of the paroxysm of the preceding evening; thereby producing a form of disease, that could only be distinguished, by the most careful attention, from one long paroxysm of thirty hours or more: yet this was in fact, only a degenerated double tertian, the paroxysms of which became accidentally mixed with each other.

The types which I have described above are all the varieties, that I observed in the fevers of the West Indies; yet more extensive experience might have, perhaps, brought to my knowledge still further com-

plications: for beſides the forms mentioned in the preceding pages, I had the opportunity, in the ſouthern provinces of North America, to ſee ſeveral inſtances of the triple tertian, properly ſo called. In forty-eight hours there were three ſeparate paroxyſms; all of them differing from each other, but correſponding with others that followed.—They were eaſily traced, as the acceſſions in that climate were uſually diſtinguiſhed by a cold fit. In Jamaica, on the contrary, complications and irregularities were marked with difficulty. The anticipation of type among other things occaſioned conſiderable perplexity. Thus the ſingle tertian, whoſe regular period is forty-eight hours, often completed its revolutions in forty-ſix. But though the paroxyſms frequently returned one hour or even two hours ſooner than the uſual time; yet theſe anticipations were ſeldom longer, unleſs the diſeaſe was of a malignant nature, or diſpoſed to change to a continued form. In either of theſe caſes anticipations of twelve or fourteen hours were not uncommon. Of the different forms of fever the anticipations of the ſingle tertian were the longeſt and moſt remarkable. It was obſerved, indeed, that the evening paroxyſm of the double tertian very generally returned before the uſual hour; yet this return was ſeldom more than an hour at once; nor did the time of invaſion in the whole courſe of the diſeaſe, ſo far as I have obſerved, ever go beyond twelve at noon. The morning paroxyſm, on the contrary, frequently poſtponed; yet I have likewiſe obſerved it ſometimes to anticipate ſix or eight hours at one time; by which means, it encroached on the paroxyſm of the preceding evening, and produced the diſeaſe which Cleghorn has diſtinguiſhed by the name of Semitertian. The obſervation of this fact does not ſeem to have eſcaped Avicenna.

There undoubtedly are accidental circumſtances, which have ſome effect in accelerating or retarding

the return of the paroxyſm for a ſhort ſpace of time; yet it would appear, upon the whole, that the cauſe, why a fever anticipates or poſtpones, depends upon ſomething that is peculiar to the particular nature of the diſeaſe. Thus, a fever, which once begins to anticipate, generally goes on anticipating through a great part of its courſe; a certain proportion being frequently preſerved between the anticipations of the different paroxyſms. This was particularly the caſe in the ſingle tertian of America. The paroxyſms of the fevers of that country often anticipated to a certain point, by ſhort anticipations, which bore a regular proportion to each other; whilſt they were ſometimes likewiſe obſerved to poſtpone, in the ſame gradual manner, towards the termination. This ſeldom happened in the fevers of the Weſt Indies.

The anticipations I have mentioned often occaſion perplexity in tracing the types of fevers; yet the complications which ariſe in the progreſs of the diſeaſe, have a ſtill more conſiderable effect in embarraſſing the appearances of regularity. Fevers which begin as ſingle tertian, often continue ſuch through the whole of their courſe; yet it ſometimes likewiſe happens, that complicating fevers make their appearance on the even days for inſtance, and continue longer, or terminate ſooner than the original complaint, in ſuch manner, as if their exiſtence no way depended on it. To be able to diſtinguiſh thoſe complications from the anticipations of a ſingle type is frequently very uſeful, and a knowledge of it may, in general, be attained with a good deal of certainty. Thus for inſtance, if the diſeaſe is moderate in its ſymptoms, and without ſuſpicion of malignity, the appearance of a paroxyſm twelve or fourteen hours before the uſual time, eſpecially if there is no material difference in the nature of the ſymptoms, gives reaſon to ſuſpect that the premature return is in reality the complication of another fever. On the contrary, where the diſeaſe

has betrayed signs of malignity, or where the symptoms differ from those of the former paroxysms only in a greater degree of violence, there is then reason to apprehend that this appearance is only an anticipation. On the subject of complication of type, some curious observations may be found in the writings of Galen and Avicenna. The opinions of those authors, indeed, are often mixed with whimsical theories; yet in many respects they are fundamentally true and highly interesting.

It would be a matter of some utility could we learn to foretel, from the nature of the types, the accidents that are likely to happen in the progress of the disease, or to form a probable conjecture of the event. Something certainly may be gained if we observe with attention. Thus I may remark, that I never found anticipations of one hour or even two to be of much consequence in the fevers of Jamaica, particularly if they happened at an early period; yet if they were longer, or did not happen till after a long continuance of the disease, they often indicated an approaching crisis. On the contrary, where the paroxysm anticipated twelve or fourteen hours at one time, there was always suspicion of danger, at whatever period this might happen. It either indicated danger and malignity, or a disposition in the fever to change to a continued form. Anticipating fevers were likewise observed to be more disposed to terminate of themselves, and likewise to terminate more speedily, than those which steadily preserved the same hour of return. This seems to have been known to the ancients.—But farther, as anticipating types are generally a sign of increasing violence, though of a more speedy termination; so the postponing of the paroxysm, has usually been allowed to indicate a disease, whose violence has begun to decline. Such is the common observation; nor have I ever found it to be otherwise; unless in some cases of weakness and im-

paired ſenſibility, where the fatal paroxyſm did not come on till after the uſual hour of attack.

I am afraid I may appear to many as unneceſſarily minute, on a ſubject, which is not in general conſidered as of much importance; yet ſtill I cannot forbear to mention ſome circumſtances of connection, between the hour of invaſion and the future type of the fever, which appear to be not only curious, but uſeful; and which, ſo far as I know, have not been taken notice of by any preceding writer. Galen, it is true, boaſts the knowledge of foretelling, from the appearance of the firſt paroxyſm, the nature of the future type of the diſeaſe; yet the hour of invaſion is not included among the number of thoſe ſigns, from which he has drawn his information. The rules, indeed, which he has left us are not altogether without foundation; yet they are by no means to be depended on alone. They have, in fact, ariſen from theories of bile and phlegm, rather than from accurate and careful obſervation. There is not, perhaps, any one criterion on this ſubject that applies equally in every climate. Thoſe rules, which I thought I had diſcovered, are only local. They varied in the different parts of Jamaica, and in moſt parts of America did not apply in any degree. Having adopted, on my arrival in the Weſt Indies, the method of writing down and analyzing ſuch caſes of fever as came under my care, the difference of hour, at which fevers of a different type uſually commenced, ſtruck me as a matter of no ſmall curioſity. The ſingle tertian, for inſtance, was conſtantly remarked to begin in the forenoon, uſually between the hours of eight and eleven; whilſt thoſe forms of diſeaſe, that were evidently quotidian, or ſtill more continued as conſtantly began in the evening, generally from four to eight. This ſeemed to be a fixed diſtinction; it is an important one; and ſo far there was no ambiguity: but it was extremely difficult, perhaps impoſſible, to

discover signs at an early period, which were capable of distinguishing the fever, which continued simple in its form throughout, or which became complicated at a certain period of its course. I have often made a fortunate conjecture; but I believe it would occasion embarrassment, rather than afford information, were I to attempt to describe those circumstances, which sometimes determined my opinion. They must, in fact, be learnt from actual observation. I have just now mentioned, that it is extremely difficult to distinguish the single from the double tertian, by the appearances of the first paroxysm, when the morning fever of this last form begins the disease; so neither is it easy to distinguish the double tertian from the quotidian, when the illness commences with the evening paroxysm of that complicated type. This is a case, indeed, as far as my experience goes, rarely happens; yet where it does happen, the circumstances, with which the evening fever is usually attended, mark a further distinction. The quotidian commences usually by a distinct and regular paroxysm; the evening fever of the double tertian, for the most part, only by a slight indisposition.

This connexion which I have just mentioned, between the hour of invasion and the type of the fever, was observed constantly at Savanna la Mar in Jamaica; but the same rules did not by any means hold true in the different parts of the continent of America. The most usual hour of the invasion of the single tertian was twelve at noon in that country; though in some cases the paroxysm came on so early as ten in the morning, or so late as two in the afternoon. Of the other forms I cannot speak with any certainty.

But besides the different hour of invasion of the different types, I must likewise take notice of some other circumstances, that seemed to be connected with the various forms. The duration of the paroxysm for instance, was usually longer in the single tertian than in the double tertian, or quotidian; and

of the double tertian, the paroxyſm of the morning was uſually longer than that of the evening; and beſides being longer, was generally of greater violence, at leaſt in the beginning of the diſeaſe. The cold fit was likewiſe more remarkable in the ſingle tertian, than in the other forms of fever.—I do not ſpeak of the quartan, of which I know but little. Of the bilious vomiting, ſo much inſiſted on by Galen as a diſtinguiſhing mark of the ſingle tertian, I am at a loſs to ſpeak poſitively. I can, indeed, affirm that I conſtantly obſerved ſuch evacuations to be more frequent in the different forms of the tertian, than in thoſe that appeared to be quotidian, or that approached nearer to a continued type.

Such were the types of the fever of Jamaica, and ſuch were the principal circumſtances that appeared to be connected with them. The types of fevers it may be obſerved ſeem to be modified by climate; and there are not perhaps two different countries, or even diſtricts of country, in which they are exactly alike. The hour of invaſion of the ſame form of diſeaſe was different in Jamaica, and on the continent of North America; neither does it appear to have been exactly the ſame in Jamaica, and in the different iſlands of the Mediterranean. The prevalence of certain forms in certain climates, and the various changes of the form according to the changes of the ſeaſon, is a matter of curioſity, and of conſiderable importance in the hiſtory of the diſeaſe. In Jamaica for inſtance, for one ſingle tertian, there were at leaſt three double ones; whilſt in America, the ſingle tertian bore the proportion of ten to one, perhaps, to all the other forms. In the higher latitudes of this country, the ſingle tertian was almoſt the only form which was ſeen, in the winter months and in ſpring; yet in the ſouthern provinces, particularly in the heat of ſummer and ſometimes in autumn, the double tertian, and even ſtill more complicated types were by no means uncommon.

I have thus endeavoured in the preceding pages to describe the types of remitting fevers, as they occured to my own observation. I have likewise attempted to compare my own descriptions with those of other authors; so that the reader may, in some degree, be able to attain a systematic view of this species of disease, as it appears in the different parts of the world. The labour I am aware will be reckoned superfluous by many; and a discussion on types, will probably be considered as partaking too much of the school of Galen, who is held in contempt by the writers of the present day. I by no means contend for the infallibility of Galen or the ancients; yet I cannot help believing, that though they have left us much false and superfluous theory, they have likewise left us many valuable observations, on the nature and progress of febrile diseases, which the moderns seem to have neglected. Though the theory of Galen on the present subject is probably ill founded, his observations are certainly exact; and from what I have myself seen, no assertions, which will convince me, that an intimate acqaintance with the types and periods of fevers, is not an essential knowledge to the practitioner. It is, indeed, the first step; and it is a step of such importance, that our future progress will neither be satisfactory nor safe, unless it is properly understood. I may be allowed to speak from my own experience. It fell to my lot to have the charge of men's lives at an early period of life. I had then no knowledge of the types and periods of febrile diseases, and I must not conceal, that the method of treatment I pursued, and the returns of the disease so often interfered, that though actual harm was seldom done, yet many opportunities of doing good were certainly lost. The patient, if he had the least penetration, could not, sometimes, avoid seeing, that, though I might be acquainted with the common routine of practice, I was ignorant of the nature and progress of the disease, from which I had undertaken to relieve him.

CHAP. III.

CRITICAL DAYS IN FEVERS.

THE critical days, which are intimately connected with the types and periods of fevers, come properly to be considered in this place. The subject is important, and though very fully discussed by medical writers, does not as yet appear to have been satisfactorily explained by any one. If we attempt to trace the doctrine to its source, we shall find the first mention of it in the writings of Hippocrates. The followers of this author's opinions are numerous; and the endeavours, they have employed in attempting to establish his system, have been strenuous and unremitting. But critical days have unfortunately afforded a field for controversy in all ages; and there still are many who maintain, and others who as confinently deny the existence of any such power in affecting the termination of febrile diseases. In the midst of this perplexity, a man who has had no experience of his own is totally at a loss, which way to turn. There are great names on both sides of the question, but clear and decisive evidence on neither. A detail, therefore, of such observations as promise to remove many of those circumstances of embarrassment, cannot fail of being useful, and I hope acceptable to the public. I am aware, indeed, that an attempt to explain a mystery, which has hitherto eluded the researches of the most eminent physicians, will not probably escape censure:—In an obscure man, perhaps, it may be deemed arrogant; neither is it altogether without hesitation that I undertake the discussion; though very unequivocal proofs of the truth of the principles on which I proceed, arise from a view

of the cases of fever which came under my care, during the time I lived in the West Indies. The principle on which the critical days in that climate depend, appears from the facts found in the following pages to be indisputably established; the deviations are satisfactorily accounted for; and the fundamental rules, it is presumed, are such as may be extended to every climate on the globe. But, I shall state in a few words the leading circumstances, which occurred to me on the subject. If they afford not light enough to remove all the difficulties, it is hoped they may at least point out a road, by which these difficulties may in future be removed.

It may not be improper to remark in the first place, that I had heard of the doctrine of critical days in fevers before my arrival in the West Indies; yet I may likewise observe, that it was a doctrine, which I had only heard of by name. I had no knowledge of it, and I soon found that the idea was treated with ridicule by practitioners, who very generally supposed the course of the fevers of Jamaica to be cut short by bark, or other powerful means. Influenced, perhaps, by the authority of older men, I found myself disposed to acquiesce in the common opinion, that this doctrine was only one of the fanciful theories of the schools; yet it was not long before I acquired a different view of the subject. I soon observed that fevers sometimes ceased before a single grain of bark was given; sometimes after a few doses, and sometimes not till after several ounces. The observation of this fact did not fail to undeceive me. Under the circumstances I mention, it would have been the height of obstinacy to have persisted in believing, that the cure of the fever was in reality owing to the power of this celebrated remedy. But though it was soon evident, that the termination of the disease depended on some other thing than that which was generally supposed; yet a considerable time past over, before I was able to determine what this something actually

was, or before I was able to ascertain the laws which it obeyed. I soon discovered, indeed, that fevers had a general tendency to terminate on particular days; but it was not till the year 1776, that I discovered the proportion those days bore to each other, or the sources of the many deviations, which occurred. The subject seemed to be important;—and I felt an eagerness to determine a question, which hitherto had been fruitlessly pursued. With this view I wrote down with care and attention every case of fever which I met with in the year 1776 and 1777; and in looking over the memoranda sometime after, found, that the critical days bore to each other the following proportion: viz. of sixty cases, which terminated favourably, ten terminated on the third, ten on the fifth, twenty on the seventh, ten on the ninth, five on the eleventh, three on the thirteenth, and two on the seventeenth. Of nine which terminated fatally, one terminated on the sixth, one on the seventh, six on the eighth, and one on the tenth. These facts are precise and determinate; but I must not forget to mention, that if we are guided wholly by obvious appearances, there sometimes occur circumstances, which occasion embarrassment. Thus in the present instances, I not only reckoned the time by the periods, or revolutions of the disease, but I likewise simplified the complicated types; that is, I reckoned every revolution of the single tertian as forty-eight hours, though it was often compleated in less; whilst I considered the corresponding paroxysms of the double tertian, only as the same disease. It may also be farther remarked, that the disease, which was the subject of this investigation, was of a regular, remitting form. Paroxysms and remissions were always discernible, and signs of crisis were generally distinct.

The state of the critical days, as represented above is literally exact, where the complicated types were simplified, and where the time was reckoned by the periods of the disease; but I must likewise observe, that

unleſs this method of calculation was adopted, there occured numerous inſtances, which ſeemed to deviate from the general rule. In the firſt place, if the type of the fever was ſingle tertian, which neither anticipated nor poſtponed,—and with paroxyſms which did not exceed twelve hours in duration, the criſis was uniformly on an odd day: yet if the type anticipated, and the ſum of the anticipations, in the courſe of the diſeaſe, was equal to twenty-four hours, the criſis was then neceſſarily removed to an even day, if the time was reckoned by the natural day; though ſtill on an odd day, if reckoned in the manner which has been mentioned above. In like manner, if the type poſtponed, while the duration of the paroxyſm exceeded or amounted to 24 hours, the criſis was neceſſarily protracted to an even day. But this was a caſe, which ſeldom happened. In fevers likewiſe of the double tertian type, the type which prevailed principally at Savanna la Mar, there occurred much ſeeming irregularity. This form of fever, as was ſaid before, ſeemed to conſiſt of two diſeaſes, which ran a ſeparate and independent courſe. Thus, if the fever which began on the odd day was critical; that is, if the paroxyſm of the odd day terminated the diſeaſe, the criſis was neceſſarily on an odd day; but if that fever, the firſt attack of which was on the even day, conſiſted of an equal number of paroxyſms with the other, or continued after that had ceaſed, the criſis was then on an even day, reckoning from the beginning of the illneſs, though ſtill on an odd day, dating from the commencement of the ſecond fever. It was the obſervation of this fact which firſt gave me the idea of ſimplifying complicated types, and of calculating the critical days by the periods of the diſeaſe. The idea may perhaps be reckoned fanciful; but experience has afforded me ſufficient proofs, and it will ſtill afford the ſame to thoſe who take the trouble to look for them, that the various types of complicated fevers

actually run a separate and independent course; a fact when established, which removes all doubt and ambiguity from the apparently varying laws of critical days in the compound forms of febrile diseases. With regard to the quotidian it remains to be remarked, that the crisis was generally on an odd day. It was likewise generally on an odd day in those that were still more continued and acute;—a fact which seems to have been well known to Avicenna. But though the rules I have mentioned are clear and uniform, I must still own, that I have sometimes met with fevers of a very continued kind, which terminated late on the sixth, or rather very early on the seventh. The disease was then of more than usual violence on the sixth:—how far this might be owing to anticipations of the paroxysm of the seventh, accumulated upon that of the sixth, is difficult to determine with certainty.

The anticipation, the postponing, and the complication of type are the principal circumstances, which usually disturb the regular critical periods in fevers of short duration; yet in those of longer continuance, there is still another cause, which deserves to be particularly attended to. In the fevers of Jamaica, especially in those which approached to a continued form, some very apparent change in the nature of the symptoms, or in the mode of action of the febrile cause, was generally obferved on the seventh, or before it. In consequence of this change, the order of the critical days was sometimes disturbed, and appearances were often produced, which seemed to contradict the rules, which we have attempted to establish. It was a common remark, that after the seventh there was less apparent regularity in the movements of nature. This, as we shall afterwards attempt to prove, was the consequence of a septenary revolution, which accidentally disturbed the regular order of the ordinary days of crisis. It is a fact of which the ancients were not ignorant; and of which I shall have occa-

ſion to make frequent uſe: viz. that a relapſe has a tendency to run over a courſe of duration equal to the original fever. This is confirmed by the authority of Hippocrates; but I may alſo add, that not only thoſe recurrences of fever, which are more properly ſtyled relapſes; but further, that in thoſe inſtances, where the diſeaſe undergoes any remarkable change in the nature of its ſymptoms, the diſorder is generally diſpoſed to continue for the ſame length of time in this new form, as it had done in the former. Thus a remarkable change of ſymptoms on the fifth was followed by a criſis on the ninth; ſometimes, perhaps, only by another change of ſymptoms on the ninth, the final criſis not happening till after another period of five days. In like manner, a change of ſymptoms on the ſeventh was often followed by a criſis on the thirteenth; or only, perhaps, by another change on the thirteenth, the diſeaſe completing another revolution of ſeven days before a final termination. That ſuch changes actually do take place at certain periods, not only thoſe caſes of fever, which have come under my own care, but thoſe related by Hippocrates, in the books of Epidemics, give ſufficient room to believe. Thus in every one of thoſe inſtances, where the hiſtory is ſo circumſtantially detailed as to leave it in our power to trace the diſeaſe in its progreſs, it will conſtantly be found, if the day of criſis deviates from the general rule, that a change of ſymptoms, often an evident renewal of fever, had actually taken place at ſome period of the courſe. In this manner, if the change of ſymptoms of which I ſpeak happened on an odd day, the odd days continued to be critical, as if no change had been; on the contrary, if the paroxyſm of the odd day completed its courſe, the remiſſion which followed was often more perfect than uſual:—a diſtinct period was marked in the hiſtory of the diſeaſe,—or in other words, there was an obſcure or imperfect criſis. But on the

day following, which was an even day, a fever with a different train of symptoms made its appearance, and ran over a course, for the most part, equal in duration to the former. If this change, or renewal of the disease happened on the sixth, a change or crisis was not expected till the tenth, if on the eighth, not till the fourteenth. I have said just now, that relapses were generally disposed to run over a course of the same duration as the original disease; yet I must likewise remark, that they were sometimes also of shorter continuance. Thus I have frequently observed a change of the nature of the symptoms on the seventh, and a final crisis on the eleventh; the renewal of the disease, instead of seven, being only of five days continuance.

The above circumstances are capable of explaining the ordinary deviations from the regular critical periods in the fevers of the West-Indies; but I cannot affirm with the same certainty, that a similar explanation will be constantly admitted in the long fevers of this country. I have however reason to believe, that changes at the septenary periods frequently take place here, and sometimes apparently disturb the critical periods of the disease. Those cases which I have been able to trace with accuracy give strong proofs of it,—I shall relate two or three of them to serve as an illustration. The first, is that of a young man, who had been ill of a fever more than three weeks before I was called to him. Two days before I saw him; and after an evident abatement of the symptoms, there happened a sudden and unexpected relapse, or renewal of the disease. Informed of this circumstance, I dated from the new attack, and calculated the critical days in the manner which has been shewn above. Minute attention discovered the type, though it was only an obscure one.—It was Semitertian, or there was an exacerbation every evening, with a more evident paroxysm

on the alternate days. A crisis happened at the period I had foreseen, but it was not final. A fever returned again in the evening, different however in type, as well as in symptoms, from the preceding. It had distinct quotidian exacerbations and an imperfect crisis happened on the seventh. But in twelve or fourteen hours, a coldness and shivering marked a renewal of the old, or perhaps the invasion of a new disease. The symptoms were not only different in their nature from the symptoms of the former; but they were likewise more violent in degree. The disease continued in this form for seven days, and the crisis, which at last was only imperfect, was soon succeeded by another renewal of fever, the beginning of which was marked by a similar degree of coldness and shivering. The symptoms of this were likewise different from the preceding, but its form was the same, and it ran over a course of equal duration. The septenary revolutions were very plain in this case. I shall relate another in which they were not so clearly marked, though they certainly did still take place. It is a case of fever with nervous symptoms. On the seventh a sediment appeared in the urine, some drops of blood fell from the nose; and the abatement of fever was very evident; yet it did not last long. The disease recurred again on the eighth, and continued to increase in violence till the fourteenth. A sediment then appeared in the urine, some drops of blood fell from the nose as before, there were two or three evacuations by stool, which had been unusual in the preceding course of the disease; and from the whole appearances I could not help entertaining some faint hopes of crisis. There was indeed an evident alleviation of the sufferings; but it lasted but for a short time. Next day every symptom was aggravated, and the powers of life seemed to suffer a gradual diminution till the twentieth, when the patient died. I do not recollect any instance of fever, where the revolutions were more ob-

ſcure than in the preſent caſe; yet they were ſtill capable of being traced. The next example I ſhall mention is much clearer. It is an inſtance of a bad fever, of no diſcernible type in the beginning, in a man who was conſiderably advanced in years. On the evening of the ſeventh there was ſome obſcure tendency to criſis. The patient was not only eaſier in his own feelings; but the eye and countenance, which had been confuſed and clouded, brightened up, and a ſmall ſediment appeared in the urine. Yet theſe favourable circumſtances were only of ſhort duration. In the courſe of the day following, all the ſymptoms recurred, and the diſeaſe acquired force till the evening of the thirteenth. The pulſe then began to riſe, and continued riſing till the morning of the fourteenth, when a profuſe ſweat was followed by a very diſtinct criſis. But ſtill this criſis was not final. The malignity of the diſeaſe, however, departed, and the complaint that remained, aſſuming a remitting form, totally diſappeared after another period of ſeven days. I ſhall only beg leave to relate another inſtance of fever, which occured to me lately, and which affords a very curious proof of ſeptenary revolutions in febrile diſeaſes of long continuance. A young man had been ill of a fever about a fortnight before I was called to him. At the time I firſt ſaw him, the ſymptoms were very violent; but having abated conſiderably in the courſe of a day or two, I began to entertain hopes of a ſpeedy recovery. The complaint was almoſt entirely gone, when a new train of ſymptoms unexpectedly making its appearance, raged with violence for a day or two, and then declined gradually as the other had done. I again looked for ſigns of criſis, when another acceſſion on the ſeventh from the former attack, brought matters into ſtill greater danger. Theſe ſymptoms, though of a different nature from the former, were violent in the beginning; but they ſoon began to abate, and had

almoſt diſappeared, when the attack was once more renewed on the following ſeventh. In this manner the diſeaſe went through nine ſeptenary revolutions; and it is ſomewhat remarkable, that the ſymptoms, which marked the new acceſſion, were always different from thoſe of the acceſſion immediately preceding. In one, the diſtinguiſhing ſymptoms were a moroſe and ſtern ſullenneſs, in another, delirium, tremors and ſubſultus tendinum,—and in the third, copious liver-coloured ſtools. Theſe were three times ſeverally repeated. It deſerves, however, to be remarked, that the period of the acceſſions was ſhortened before the termination of the diſeaſe. After it had continued nine weeks in the manner I have deſcribed above, there were two acceſſions of five days each; after which all traces of fever diſappeared.

It is ſufficiently plain from the facts which I have mentioned in the preceding pages, that the more uſual irregularities in the order of the critical days, proceed generally from overlooking the type in periodical fevers, or from neglecting to attend to ſeptenary, and other revolutions, in ſuch as approach more nearly to a continued form. Theſe are the general cauſes of apparent irregularity; yet beſides theſe, there are ſtill ſome others, which muſt not be paſſed over without notice, as they occaſionally have the effect of producing apparent deviations. Thus it often happens, that a diſeaſe, which appears to be continued in the beginning, changes to remitting after a certain duration. The change is uſually on an odd day, and on the day following the firſt paroxyſm of the remitting form makes its appearance, the termination of which may be expected on an even day, if we date from the beginning of the illneſs, though ſtill on an odd day, if we date, (as perhaps we ought to do) from the time this change in the circumſtances of the diſeaſe took place. To this we may add, that thoſe complicating fevers, which, happening at various dif-

tances of time, ſometimes terminate ſooner, ſometimes continue longer than the original complaint, frequently diſturb in appearance the general regularity of the critical periods of nature. It happens, perhaps, from a ſimilar cauſe, that a paroxyſm of an unuſual kind ſometimes terminates the diſeaſe, and apparently diſturbs the regular periods of criſis This has occurred to me ſeveral times in practice; and it happened twice in my own perſon. The ordinary paroxyſm declined after the uſual duration; a new one ſucceeded of uncommon violence, and very different in its nature from the former. Its courſe was of long continuance, and it finally terminated the diſeaſe.

The above facts enable us to explain ſatisfactorily every circumſtance, which relates to critical days in fevers, where the criſis is clear and decided; yet I muſt ſtill own, that as I have ſometimes met with fevers where marks of criſis were ſcarcely perceptible ſo it would be raſhneſs, in ſuch caſes, to ſpeak poſitively of the order of the critical days. The patient might, in ſome meaſure, be ſaid to wade through the diſeaſe; the changes from day to day being ſo very ſmall, that it required more diſcernment than I can boaſt of to mark them with preciſion.

The obſervations I have related, and the rules I have attempted to eſtabliſh, for the better explanation of the doctrine of critical days in fevers, were formed at a time when I had no knowledge of the opinions of preceding authors. They may therefore better claim exemption from bias in favour of one ſet of writers, or prejudice againſt another. They are indeed no more than an analyſis of facts, which were collected with every poſſible care, which are ſufficiently circumſtantial, and which ſpeak beſt for themſelves. They contain, (if I do not view them with a partial eye,) ſuch information, as may lead to a ſatisfactory explanation of this myſterious and long

disputed doctrine.—I must only beg leave to add, that though I have everywhere mentioned the pre-eminence of particular days in terminating fevers, yet it must not be understood, that this power depends on a particular quality of the days, merely as such. It depends more evidently on a certain number of revolutions of the disease, in consequence of which, the fever from something we do not in the least understand, seems disposed to terminate finally, or to suffer a change in its mode of action. This therefore brings us to the conclusion, that the critical periods are improper calculated by the natural day. The doctrine, in short, can only be rendered consistent by attending to the periods of the disease, by simplifying complicated types, and by marking those septenary or other revolutions, which happening at different distances of time, occasion an appearance of irregularity which does not exist in reality.

Having related the result of my own observations on critical days in fevers, I shall now endeavour to bring under one point of view, the substance of what has been written on the subject, by some of the most celebrated of the ancient, as well as modern physicians. That certain days, or that portions of time comprehended in a certain number of days, had obviously a power of producing changes on the human frame, appears to be an observation of high antiquity; but as a medical doctrine, we are unable to trace it farther than the days of Hippocrates. Hippocrates has treated very fully of the critical periods of fevers, in various parts of his works; and upon the whole, has amassed a considerable body of information; though with less precision, perhaps, than has been generally imagined. The cases of the Epidemics, which we naturally consider as the materials from which he formed his general doctrine, have some obvious and great defects. The date is seldom clearly ascertained, and the mode of calculating the time, does not seem to be fixed. If

a fever, for inſtance, begins in the evening, or in the courſe of the night, the day following is generally reckoned the firſt day of the diſeaſe, by this author.—But this is not all.—Some of the caſes are plainly related from memory; and others are only parts of caſes, related by different perſons. This want of accuracy, where it is ſcarcely poſſible to be too circumſtantial, neceſſarily breeds confuſion, and produces an appearance of irregularity, which does not actually exiſt. Hence we find inconſiſtency in the general doctrine, as delivered in different parts of the works, which have been aſcribed to Hippocrates; at the ſame time, that there is a want of that circumſtantial detail in the particular parts, from which only we can be enabled to form an opinion. I have read over with much attention the caſes of fevers, recorded in the Epidemics; but I frequently found myſelf unable to trace the diſeaſe in its progreſs. Though evidently ſubject to periodical movements, it was not always in my power to lay hold of the type; yet wherever it was poſſible to attain this exactneſs, I have the ſatisfaction to add, that I conſtantly found the movements of nature to be uniform. They were the ſame in the iſlands of the Archipelago, as in the iſland of Jamaica.—If they appeared in ſome inſtances to be different, it was perhaps principally owing to this, that the Greek phyſician had left ſome part of the diſeaſe undeſcribed.

From what I have juſt now ſaid, we can have no heſitation in concluding, that the opinion of Hippocrates, on the ſubject of critical days, is neither preciſe in any one part, nor conſiſtent in the whole. The doctrine, however, in its beſt digeſted form, is the following: viz. That odd days have a remarkable power in terminating fevers; but more particularly, that the great critical revolutions happen at quaternary periods. Thus the moſt eminent critical days, are the fourth, the ſeventh, the eleventh, the four-

teenth, the seventeenth and the twentieth. This is the general form of this Hippocratic doctrine; yet in this form, it bears contradiction to observations that are found in various parts of that ancient author's works. The fifth and ninth are excluded by this arrangement, from the number of the critical days; though there are numerous examples of their great power, in terminating febrile diseases.

The doctrine of critical days, which appeared first in a regular form, in the writings of Hippocrates, found numerous and respectable advocates among the ancient physicians. Diocles of Caryſtus, Philotimus, Heraclides of Tarentum, &c. all bore testimony to the general truth of the observation; but their writings being unfortunately lost, we are now ignorant of the particular facts and arguments, by which they attempted to support their opinions. Indeed, from the time of the Persian invasion of Greece, till the Roman arms penetrated into Asia, a period of near four hundred years, we know of no opposition to to this fundamental doctrine of the Coan Sage: But in the time of Pompey the Great, an author arose, who endeavoured to establish his own fame on the ruins of this favourite system of his predecessors. Asclepiades, who was a man of a bold and daring genius, not only rejected this apparently well founded doctrine of the ancients, but treated the idea of it with ridicule. His arguments are ingenious and acute; but they fall short of the truth. The paroxysms or exacerbations, as he justly observes, sometimes change to the even days, and consequently the crisis: yet this, if properly understood, does not destroy the generality of the rules;—if the method of calculating the time, by the periods and revolutions of the disease, be adopted, the difficulty is perfectly removed. But though this fact in reality, was not unknown to Asclepiades; yet it does not appear, that he understood the application of it. I may add, that

he has precipiately rejected the doctrine, from the very circumstance which establishes its reality.

We do not meet with any thing very material, on the present subject, between the time of Asclepiades, and the days of Galen. There appears, indeed, to have been many, who adopting the opinion, and copying the arguments of the eloquent Bethynian, denied altogether the existence of critical periods in fevers; whilst others, recurring to the doctrine of Hippocrates, maintained their reality with no less obstinacy. But we are now in a great measure ignorant, if those writers attempted to support their opinions by any new facts, or new arguments. Among other misfortunes, we must regret particularly, that the treatise of Aretæus on fevers is lost. From what we know of this author's industry we might have reasonably expected original information on the subject in question.

Galen, whose fertile and exuberant genius left no path in physic unexplored, has written fully on this celebrated doctrine. He has professedly adopted the opinion of Hippocrates, and laboured much to explain and confirm it; but unfortunately, he has oftener overwhelmed the subject with diffuse and tedious reasonings, than illustrated it by proofs from experience and actual observation. Upon the whole, however, amidst much superfluous and unmeaning matter, we find not only useful information, but a more systematic arrangement of facts, than is any where to be met with. He has attempted to fix with more precision the date of invasion; he has estimated with more accuracy the critical power of the different days; and further, has hinted obscurely, that the time will be calculated most conveniently by the paroxysms or revolutions of the disease. In short, this author, no less than Asclepiades, was sufficiently acquainted with the principal truths, which give consistency to this doctrine; but it is evident, that he

did not underſtand the full extent of their application. He was conſtantly biaſſed by the theory of a quaternary period; as without this predilection, it is not eaſy to conceive, how he ſhould have conſidered the fourteenth, as critical of tertians, where the paroxyſms happen on the odd days, and where the termination, as he acknowledges, conſtantly follows the ſolution of a paroxyſm. The latitude likewiſe which he aſſumes, in explaining the apparent irregularities, is much too great. If we are permitted to reckon either the beginning or the termination of a paroxyſm, as the critical period, according as it ſhall beſt ſuit our theory, it is eaſy to elude the moſt poſitive teſtimonies of experience. Yet, notwithſtanding theſe defects, the different tracts of Galen on this ſubject, deſerve to be carefully read. The facts they contain, though ſometimes miſapplied, are often important; and though we are not always ſatisfied with the reaſonings of the author, we are aſtoniſhed at the amazing maſs of learning and knowledge found in his works.

There is little new information, on the ſubject of critical days, to be met with in the writings of thoſe Greek phyſicians, who were poſterior to the time of Galen. Ætius Amidenus, indeed, brings into narrower compaſs the ſubſtance of the doctrines of his predeceſſors. He mentions likewiſe, the moſt material of thoſe circumſtances, which influence the deviations from the regular criſis; but it is evident, that he has not ſufficiently underſtood their application. Alexander Frallianus, who was an excellent practitioner, and a man of long experience, paſſes over this ſubject without particular notice; and though Paulus of Ægina has detailed the opinions of Galen in a more compreſſed form, than they are found in the original author; yet he has not added any new obſervations of his own. From the manner, indeed, in which he ſpeaks, of the peculiar virtue of

the ſeventh and fourteenth, we ſhould be apt to believe, that he is not altogether free from prepoſſeſſion in favour of the Pythagorean numbers.

It was reaſonable to have expected information, on the ſubject of critical days, from the writings of the Arabian phyſicians. The Arabians inhabit a country, were the periodical movements of nature are perhaps more clearly marked, than in our northern latitudes. Some diſtricts of their country likewiſe were famous for the ſciences at an early period, though it does not indeed appear that much of this knowledge deſcended in a direct channel to the Arabians of the preſent times. The Arabian phyſicians, in many inſtances, enriched medical practice with new forms of remedies; but they have for the moſt part only adopted the theoretical doctrines of the Greeks, particularly of Galen. Avicenna, the moſt famous among their phyſicians, and undoubtedly a great man, has Galen conſtantly in his eye: in ſhort, he has done little more on the ſubject of critical days, at leaſt, than merely tranſlate the opinions and arguments of the celebrated Greek. He attempts, indeed, to be more explicit in aſcertaining the date of invaſion; but he does not in fact, go much beyond his predeceſſors;—hinting only obſcurely, that the critical days ought to be calculated from the proper formation of the type, or the diſtinct invaſion of the fever. He has added, however, that the odd days, are properly the critical days of the ſingle tertian, and that the eleventh of courſe, obtains rank of the fourteenth in this diſeaſe.

There are many authors, who have written on this ſubject, ſince the arrival of ſcience in Europe; but there are few that I have met with, who have thrown light on it from their own obſervations. The moſt of them have borrowed the opinion from Hippocrates; and accordingly have attempted to eſtabliſh the truth of it, on the facts which are found in

the writings of that author; facts, which, on enquiry, will scarcely be found to be accurate enough to be made the basis of a general doctrine. It would be time ill spent, to enter into a detail of the arguments of this numerous list of writers; who, in reality, have oftener attempted to support their opinions by the authority of Galen and the ancients, than by the facts which might have been found in their own experience. From writers, however, of this description, it would be unjust, not to separate Hoffman, an author, who has related with candour the result of his own observations, in a practice of forty years and upwards. The facts which Hoffman mentions, throw considerable light on the subject; yet still they do not remove all the difficulty. They neither enable us to form an estimate of the power of the different critical days; neither do they at all assist us in comprehending the cause of the deviations. There are probably other modern authors besides Hoffman, who have treated of the power of critical days in fevers; but, except Dr. Cullen, I have not met with any one, who has left any observations which deserve much notice. This celebrated physician is a warm advocate of the ancient doctrine of critical days. He subscribes professedly to the arrangements of Hippocrates; though he adds likewise the result of his own observation, in the various kinds of fevers of this country.

The most eminent of the ancient and the most systematic of the modern physicians, all agree in ascribing to certain days a particular power in terminating; yet they do not so perfectly coincide in the arrangement they have given of those days, or in the causes they have assigned for the particular pre-eminence. The inconsistency of Hippocrates has, perhaps, been in some measure the source of this diversity of opinion. In one place, this author has ranked the twentieth as the proper critical day in fevers; in some others, this power is attributed to the twenty-

firſt. That the twenty-firſt is properly the day of criſis, was the opinion of Archegenes and Dioclès; that it ſhould be ſo, is not inconſiſtent with the general principle of the Hippocratic doctrine; viz. the movements of a quaternary period. So far is clear; but as it was obſerved by Hippocrates, as well as by other authors, that the twentieth was ſtill more frequently a day of criſis than the twenty-firſt, a προσθεσις, on the fourteenth, was introduced to account for this apparent deviation from the general rule. This idea of προσθεσις, or accumulation of one period on another, which is mentioned in the writings of Hippocrates, originated perhaps in the doctrine of Pythagoras. It is adopted by Galen, and it appears in reality to be occaſionally true; yet it can never be conſidered as an eſtabliſhed principle in the movements of febrile diſeaſes. By means of ſuch accumulation, however, Galen has attempted to eſtabliſh the pre-eminence of the twentieth, which he conſiders as the real critical day of Hippocrates. That the twentieth—(not the twenty-firſt) is actually the critical day of Hippocrates, is likewiſe decidedly the opinion of Dr. Cullen, who, going a ſtep farther than his predeceſſors, endeavours to ſupport his aſſertion by ſome arguments, which are entirely new. This ingenious author hazards the bold conjecture, that the appearance of the twenty-firſt, in the writings of Hippocrates, has ariſen wholly from accidental error in the original manuſcript: but with all due deference to ſuch reſpectable authority, I muſt beg leave to ſuggeſt, that the twenty-firſt occurs too frequently in thoſe writings, which have been aſcribed to the Coan Sage, to give countenance to the opinion, that it owes its place, as a critical day, to careleſs error. The other argument is more ingenious; but perhaps not better founded. This writer has ventured to maintain, that the type of febrile diſeaſes changes to quartan after the eleventh; but I can ſee

no good reaſon for the ſuppoſition. Medical writers have repeatedly noticed inſtances of criſis, on the thirteenth, and fifteenth; even my own experience, narrow as it has been, furniſhes me with ſufficient evidence, that criſis actually do happen at the above-mentioned periods.

As thoſe days, which have been chiefly conſidered as critical, are now ſuppoſed to be ſufficiently known, it will not be ſuperfluous in the next place, to take a ſhort view of the cauſes, on which the particular pre-eminence has been thought immediately to depend. The quaternary period, which in reality is a period of four, and a period of three days ſucceeding each other alternately, is the general principle aſſumed by ancient phyſicians, to explain this arrangement. But if we continue to purſue the undiſturbed movements of a quaternary period, we ſhall bring the eighteenth and twenty-firſt into the order of critical days, rather than the ſeventeenth, and twentieth. The contrary is in fact the caſe. To obviate therefore this difficulty, or to reconcile obſervation with theory, a προσθεσις has been ſuppoſed to take place on the fourteenth. That a προσθεσις, or as it may be tranſlated, the accumulation of the beginning of one period on the extremity of another, frequently takes place, cannot be denied; but its appearance is not determined by a fixed law. It is obſerved on the ſeventh, on the fourteenth; in ſhort, on any day whatever. The quaternary period, with προσθεσις on the fourteenth, is the only principle employed by the ancients for explaining the uſual arrangement of the critical days; yet I muſt obſerve, that it is capable of doing this, only in a very imperfect manner; it totally excludes ſome days of very conſiderable power. Dr. Cullen, ſenſible, perhaps, of this defect, ſuggeſted that there was a chance from the tertian to the quartan type on the eleventh. This change, it muſt be confeſſed, explains with perfect plauſibility

the pre-eminence of the fourteenth, ſeventeenth and twentieth; but there is the ſtrongeſt reaſon to believe, that it does not in fact take place. I mentioned before, that inſtances are recorded by medical writers of criſis, which have happened on the thirteenth, fifteenth, and the other days, which are not included in the quartan period; and I can add from my own experience, that where the diſeaſe was of ſuch a kind, that a type could be clearly traced; no ſuch change, as this author has ſuggeſted, was ever ſeen.

Having ventured to declare, that the cauſes, which have been hitherto aſſigned for the pre-eminence of certain critical days in fevers, are extremely defective; the facts, which I have mentioned before, it is preſumed, may enable us, if they are properly underſtood, to give a more ſatisfactory explanation of this ſingular phenomenon. There are few people of experience and obſervation, who do not know that the tertian is the moſt prevailing type in febrile diſeaſes. This, at firſt ſight, gives a general pre-eminence to the odd days; but though the tertian period prevails very generally in fevers, yet it muſt alſo be remembered, that theſe revolutions are ſometimes completed in a ſhorter ſpace of time than the regular period; whilſt the types are frequently found to be doubled, or even more variouſly combined. In conſequence of theſe accidents, apparent irregularities are often produced in the order of the critical days; though they may be all ſatisfactorily accounted for, by calculating the time by the periods of the diſeaſe, or by ſimplifying thoſe types which are more evidently complicated. By attending to the circumſtances I have mentioned, all the difficulties may be eaſily removed in periodical fevers; but as numerous inſtances of fevers occur, where no type can be clearly traced; ſo it is neceſſary in ſuch caſes to ſeek for ſome other principle, which may be capable of explaining apparent irregularities. There very ſeldom

perhaps happens an inſtance of fever of long continuance, where the ſymptoms do not undergo ſome change in the courſe of the diſeaſe. Thoſe changes or revolutions are generally at conſiderable intervals, frequently at an interval of ſeven days. The circumſtances by which thoſe changes are indicated, are not by any means obſcure; and, perhaps, there would not be great error, if we conſidered them as the commencement of a new complaint; at leaſt by conſidering them as ſuch, the general principle of the critical days is preſerved conſiſtent and uniform throughout. I ſhall mention ſuch explanations as have occurred moſt frequently in my own practice. It often happened, that the ſymptoms of the diſeaſe underwent a material change on the fifth. It terminated on the ninth, or perhaps only put on a new appearance on the ninth, its final termination not happening till after another period of five days. In the ſame manner, a change of ſymptoms on the ſeventh, was followed by a criſis on the thirteenth; or if the change of ſymptoms was not obſerved till the ninth, the criſis probably did not make its appearance till the ſeventeenth. Such change of ſymptoms on the odd days, (where we may ſay with propriety enough, that one diſeaſe was accumulated upon another), there being ſeldom any previous marks of criſis, was by no means uncommon; yet it happened ſtill oftener, that the paroxyſm of the odd day declined; the original diſeaſe terminated imperfectly, whilſt a new one began the day following, which was an even day. By ſuch accidents the order of the days of criſis was changed: And from the laſt mentioned cauſe the fourteenth, as a ſecond ſeventh, becomes remarkable among the critical periods of fevers. This idea of a ſecond ſeventh occurred to me many years ago, and long before I was acquainted with the opinions of Hippocrates or of Galen. It now receives information from the teſtimony of theſe careful obſervers. There are many,

I make no doubt, who will be disposed to treat it with ridicule; but I shall combat their opinion with no other argument than a request, that they write down carefully the history of a tedious fever, and afterwards review its course without prejudice or partiality.

I now only beg leave to add, that the facts which I have mentioned in the preceding pages are circumstantial, and give room to conclude, that by simplifying complicated types, by calculating the time by the revolutions of the disease, or by beginning to date a second time from those great and remarkable changes, which happen at more distant periods, a doctrine is formed, perfectly uniform and consistent with itself. It is confirmed by every observation which I have been hitherto able to make. It is no more indeed, than an analysis of those several cases, which have occurred in my own practice; which in periodical fevers at least, has been tolerably extensive.

But though the prevalence of a tertian type, explains satisfactorily the general critical power of the odd days; and those other circumstances, which I have likewise taken notice of, account no less clearly for all the deviations, which are observed to take place; yet if we attempt to seek for a cause of this type, or of those changes, which happen at more distant, particularly at the septenary periods, our progress is soon stopt. Galen, who seldom hesitates in explaining the phenomena of nature, acknowledges here that he was unwillingly drawn to a discussion of the subject. The question undoubtedly is a difficult one; and, it is to be feared, must remain for ever unknown. In the East, where the powers of the human mind were not only earlier developed; but where men, from climate and modes of life, were led more early to observe the motions of nature, stated and periodical movements were soon discovered in the economy of the sublunary system. Egypt,

there is reaſon to believe, is one of the countries where theſe revolutions were firſt taken notice of; at leaſt it was on the banks of the Nile, that the Greek philoſophers firſt gathered the ſeeds of natural ſcience. Among the knowledge or opinions, which theſe ſages carried back to their native country, we may reckon the doctrine of the power of numbers; which though diſfigured perhaps by the metaphyſical genius of the philoſopher of Samos, has obſervation in ſome degree for its baſis. It does not concern us at preſent to enter into a particular diſcuſſion of this opinion; but as far as relates to the ſubject in queſtion, we cannot refuſe acknowledging, that the frame of man is liable to regular changes, at particular periods, comprehended in a certain number of days and hours. But though this general truth is indiſputable, yet there is no argument which leads us to ſuppoſe, that thoſe changes are, in any degree, influenced by an harmonic proportion in the ſimple number of the days. Ill founded however as this doctrine obviouſly is, it was in high faſhion with the Greeks in the time of Hippocrates; and ſeems evidently to have had ſome influence on the opinions of this author. Without ſuch a prepoſſeſſion, indeed, it is not eaſy to conceive, how he could have fabricated the ſyſtem which he has given to the world; as it by no means reſults from the facts which are found in his writings. Galen in this, as in moſt ſubjects, follows the footſteps of Hippocrates. He diſclaims, I muſt confeſs, the *power of numbers*, ſimply as numbers having any effect upon the moſt uſual days of criſis; but he maintains the influence of a quaternary period, which appears very plainly to be a remnant of the doctrine of Pythagoras. However, after exhauſting himſelf, and fatiguing his readers with a detail of uſeleſs conjectures, he at laſt ventures to conclude, that the buſineſs of criſis is to be referred ultimately to the courſe and different aſpects of the moon. The opi-

nion, like many others recorded by the Greek phyſicians, draw its origin from Egypt. It is not, perhaps, altogether without appearance of plauſibility; yet I muſt add, that if the moon has in reality any influence in this buſineſs, the laws which regulate its effects are obſcure;—indeed, not in the leaſt underſtood. The conjecture however, fanciful as it appears to be, met with the general aſſent of medical writers, till about the middle of the ſixteenth century, when Fracaſtorious, a man of ingenuity and elegant genius, attempted to ſubſtitute another in its place; though unfortunately, not a more probable one than that of his predeceſſors. This author, after a diſplay of much learning and general knowledge, at laſt ventures to conclude, that the power of the different days of criſis, depends on peculiarities in the laws of motion of the different humours, which give riſe to the different ſpecies of the diſeaſe: but with regard to this hypotheſis, it is only neceſſary to remark, that while the very exiſtence of the humours is doubted with reaſon, there can be no certainty in determining the laws of their motions. But though the opinion of Galen, and this of Fracaſtorius, are only vague and very queſtionable conjectures; yet they are the only ones, ſo far as I know, which have been offered to the public. The ſubject is too intricate, perhaps, ever to be explained. For though we clearly perceive that fevers are uſually of a ſtated duration; yet we are unable to perceive, whether this duration depends on ſomething inexplicable in the peculiar nature of the cauſe, which ceaſes to act, or changes its mode of action at a certain period; or to ſome imperceptible revolution in the human frame, which deſtroys in a given ſpace of time, that particular aptitude between the ſtate of the body and the morbid cauſe, in which the diſeaſe may be ſaid to conſiſt. This only we know with certainty, that where the febrile motions are violent and continual,

the diſeaſe haſtens to a termination; where they are languid and feeble, or ſuffer long interruptions, its duration is often drawn out to an undetermined length of time. Thus continued fevers, with inflammatory diatheſis and much viſcular excitement, for the moſt part terminate decidedly in ſeven or nine days; while thoſe with low and languid motions, with long and diſtinct intermiſſions, as the quartan, and even ſometime the tertian, continue for months, and decline at laſt by ſlow and almoſt imperceptible degrees.

It may ſeem that I have treated very fully of the critical days of fevers; yet before leaving the ſubject altogether, there is one thing ſtill which requires to be mentioned;—I mean the great proportion of fatal terminations, which happen on the even days. The even days were obſerved to be fatal in the proportion of three to one, in thoſe fevers, which came under my care during the time that I lived in Jamaica. The fact, which is curious and hitherto I believe unnoticed, was diſcovered in the following manner. That I might the better trace the progreſs of nature through the whole courſe of the fever, a ſubject which then engroſſed my chief attention, I viſited often, and ſpent much of my time in the apartments of the ſick. Among other things, I diſcovered the manner in which death more uſually approached. The natural courſe of the paroxyſm appeared generally to be finiſhed, or the action of the febrile cauſe ſeemed actually to have ceaſed. The lightning before death, as it is termed, which has been generally attributed to the laſt efforts of dying nature was frequently ſeen to take place. This was even ſometimes ſo remarkable, as to give flattering hopes of a favourable criſis; yet in a ſhort ſpace of time, the powers of life begun to fail, and at laſt were gradually extinguiſhed, like an expiring taper. —The criſis, ſtrictly ſpeaking, happened on the odd days, equally the ſame in thoſe who died, as in thoſe

who recovered; only I had inaccurately, accuſtomed myſelf to refer the critical period to that moment, were the ſigns of criſis were firſt perceived; in the other, I had conſidered it as happening at the hour of actual death. Thus it was obſerved in thoſe fevers which terminated fatally on the even days, that the powers of life, though irrecoverably exhauſted, were not totally extinguiſhed by the paroxyſm of the odd day. This paroxyſm, in ſhort, ſeemed to decline after the uſual duration. It left the body, in ſome meaſure, free from diſeaſe; but ſo completely deranged in the vital functions, that the action of living, though it often went on for a few hours, could not be continued long. In this manner, the hour of death was frequently protracted to the even day; yet death happened ſometimes on the even days, from another cauſe. The decline of the paroxyſm, which in many caſes was hardly perceptible, in others was very plain. The diſeaſe terminated; but a new one recurring, after a ſhort interval, ſpeedily put a period to exiſtence. In the mild fever of Jamaica, death uſually approached in the gradual manner I have juſt deſcribed; yet in caſes of much violence and malignity, the fatal termination was frequently on an odd day. In ſuch caſes the patient died in the height of the paroxyſm, carried off by convulſions, apoplexy, or other accident.

Thoſe authors, who, ſince the time of Aſclepiades, have denied the power of critical days in fevers, are numerous; and many of them poſſeſs conſiderable authority in the medical world. Their opinions, however, cannot be conſidered as of great influence in the preſent caſe, though they may aſſert, that they never have obſerved the pre-eminence of any particular days in terminating febrile diſeaſes; ſuch an aſſertion means but little; unleſs its author convinces us, that he has adopted a method of inveſtigation by which thoſe regular movements, if they actually ex-

iſted, could not fail to be diſcovered. Truth in the preſent caſe, can only be known from minute and careful obſervation; but a train of minute obſervation is not likely to be the work of a buſy phyſician; and one, who is little employed, has not ſufficient materials in his practice to engage his attention to a continued purſuit. I conſider it as my own good fortune, to have been placed between the two extremes of idleneſs and too much buſineſs. In the country where I reſided for ſome time, the movements of nature were generally ſo diſtinct, as to be obſerved without much difficulty; my practice likewiſe was ſufficient to employ my mind, and not more than it could comprehend eaſily; ſo that I had ſufficient leiſure to write down, and to digeſt the obſervations which I have related above. They afford, if I miſtake not, ſome facts which are preciſe and pointed; and which ſuperſede a multitude of arguments. I will not venture to ſay, that they remove all the myſtery from this dark ſubject; but I cannot help flattering myſelf, that they point out a road by which we may continue our inveſtigations with ſucceſs. The ſubject of critical days is of ſuch importance, as to demand every attention. A knowledge of it gives credibility to our art; whilſt ignorance in this reſpect is the ſource of perpetual miſtake and diſappointment. There are many phyſicians of the preſent day, who treat the idea of critical days with ridicule; but their aſſertions only afford an argument of their own precipitancy, and ſuperficial obſervation. The man in reality, who pretends to cure a fever, without a knowledge of the critical periods of nature, is no leſs preſumptuous, than the mariner, who undertakes to conduct a veſſel through the ocean, without being inſtructed in the manner of calculating her courſe.

CHAP. IV.

OF THE GENERAL REMOTE CAUSES OF INTERMITTING AND REMITTING FEVERS.

THE general remote cauſes of intermitting and remitting fevers have been ſo fully inveſtigated by ſeveral eminent writers, particularly by the induſtrious and learned Lanciſi, that little remains to be added: nor perhaps ſhould I have thought it neceſſary, even to have mentioned the ſubject, were it not to take notice of ſome opinions of the late Sir John Pringle, which appear to have been formed too precipitately; and which, I can affirm from experience, have been pernicious to the health of thouſands. It would be a very needleſs oſtentation to adduce the authority of the ancients, to prove the general ſource of the diſeaſe which is the ſubject of the preſent treatiſe. The hiſtorians, no leſs than the phyſicians of every age, do not entertain a doubt, that fevers of the intermitting and remitting kind, owe their origin to exhalations from ſwampy and moiſt grounds. Daily experience ſtill proves it; and there are few men whoſe obſervations are ſo circumſcribed, as not to know, that it is in the neighbourhood of ſwamps, and near the banks of freſh water rivers, that thoſe diſorders chiefly prevail. But though it is only in the above ſituations, that intermitting and remitting fevers are more peculiarly epidemic; yet it likewiſe deſerves to be remarked, that, independent of the particular circumſtances of ſoil and local ſituation, the endemic of champaign countries is ſubject, in a greater or leſs degree, to an appearance of periodical revolution. Mud and ſtagnant water, in every climate, poſſeſs the materials of the cauſe of this ſpecies of diſeaſe; but a combination of other cir-

cumſtances is required to give them activity. Among the principal of thoſe circumſtances, which call forth this action, we may reckon the influence of a powerful ſun. Hence, (as is commonly known), ſome ſituations, which, in the colder months of winter, are diſtinguiſhed for no particular diſeaſe, in the hot months of ſummer and autumn, are obſerved to be moſt malignantly unhealthful.

The nature of this exhalation or cauſe of fever, though it has long been a ſubject of enquiry, remains ſtill unknown. We plainly perceive it to be of various degrees of force, and in various ſtates of concentration; and we can eaſily conceive it to be variouſly modified and combined;—but we go no farther. It has been ſaid, to poſſeſs a ſeptic principle; but this alone will ſcarcely be thought ſufficient, to account for the very peculiar manner in which it affects the human race. Some other quality is neceſſarily joined with it, which our ſenſes cannot lay hold of. But though the ingenuity of man has not hitherto been able to penetrate the intimate nature of this cauſe of fever, we ſtill have it in our power, in ſome degree, to trace its effects on the human conſtitution. We plainly perceive that an habitual expoſure to it, is peculiarly *unfriendly* to the principle of life, and in a very remarkable manner ſhortens the period of exiſtence. In proof of this I mention from good authority, that white females, born and conſtantly reſiding in the lower diſtricts of the province of Georgia in America, have ſeldom been obſerved to live beyond the age of forty. Males, ſometimes approach near to fifty; while Europeans, who had arrived at manhood before they came to the country, often attain a good old age. The fact is curious, and ſhews, in a ſtrong point of view, the deleterious quality of the air of thoſe climates. But though the general nature of the country, which I have juſt now mentioned, is unhealthy in a high de-

gree; yet there are situations, in the Carolinas and Virginia, which are destructive of life in a still more remarkable manner. There is not on record, I am credibly informed, an instance of a person born at Petersborough in Virginia, and constantly residing in the same place, who has lived to the age of twenty-one. When the British army marched through this province, in the year 1781, I had the opportunity of seeing a native of this town, who was then in his twentieth year; but he was said to be the first, who had ever attained so advanced an age. He was decrepid, as if from the defects of time, and it did not appear that he could survive many months. Yet it is not a little curious, that this man had never been much confined with sickness. The residing constantly in the same pernicious air, seemed alone to have been sufficient so remarkably to accelerate decrepitude. But though the instances I have mentioned, afford sufficient proof, that this miasma is unfriendly to the principle of life; yet we are by no means instructed, as to the manner, by which it becomes so. This seems to be one of the arcana of nature; and it will profit little to prosecute it farther by conjecture. It will, however be an object of utility to mark the soils and situations in which the exhalation most abounds, and to trace the causes which heighten or lower its activity.

The history of the remote causes of intermitting and remitting fevers, with all the circumstances connected with them, having been, as I said before, so fully investigated by others, I shall only add a few cursory remarks, where the information does not seem to be sufficiently precise, or where the conclusions, which have been made, are not justifiable by experience. It is an opinion, which, though it did not originate with Sylvius de le Boe, evidently gained weight from his authority—that a mixture of salt with fresh water, as corrupting more easily, af-

fords a more noxious exhalation than freſh water alone. Lanciſi has mentioned the obſervation; and Sir John Pringle conſiders it as an eſtabliſhed fact; but the evidence, by which he attempts to ſupport his opinion, is not deciſive. It would be in vain to deny, that the neighbourhood of lakes or rivers, with a mixture of ſalt water, is often highly unhealthful; yet we may affirm with confidence, that it is ſeldom more ſo, than where the lakes and rivers are perfectly unmixed. In proof of this aſſertion, I might adduce the example of Savanna la Mar in Jamaica, or draw inſtances from the numerous iſlands on the coaſt of the Carolinas; where ſea and river water are often blended together in various proportions; to which might be added, the more particular evidence of the relative healthineſs of the banks of rivers. So far as I have obſerved, the uſual endemic was leſs frequent, and leſs formidable on the banks of rivers, after their waters became mixed with thoſe of the ſea, than before this happened; unleſs the circumſtances were in other reſpects more favourable for the production of the diſeaſe. Hence there is but little reaſon for ſuppoſing, that there actually exiſts any degree of mixture of ſalt with freſh water, at leaſt of running water, which abſolutely heightens the noxious quality of the exhalation. The above is an opinion of ſufficient conſequence to demand inveſtigation: but there is another advanced by this celebrated author, worſe founded, and of ſtill greater concern, which I ſhall likewiſe mention. From an idea that a free circulation of air, is of all things the moſt eſſential to the preſervation of health, Sir John Pringle enjoins in a very poſitive manner, not only that open ground, but that the banks of large rivers ſhould be choſen, in preference to other ſituations, for the encampment of troops. This author's opportunities of information were good; his opinion has therefore gained weight, and his advice, I am afraid,

has been often fatally followed. It would be no difficult task to produce testimonies, from both ancient and modern history, of the unhealthiness of those situations, which Sir John Pringle has thought proper to recommend; but at present I shall confine myself to that, which has more immediately fallen under my own observation. The instance I shall mention, is only a single one; but it proves so clearly the danger of encamping on the banks of fresh water rivers, as to render all others superfluous. In June 1780, the first battalion of the 71st regiment was detached to the Cheraws, where it encamped on open ground, within five hundred paces of the river Pedee. The people of the country, taught by experience, suggested the propriety of drawing back the encampment into what is called the Pine-barren, assigning as the cause of their advice, that the distance, as well as the cover of the wood, might be a security against the damps of the river, which were observed to be extremely noxious in that climate. A position in wood, accessible on all sides, would not perhaps have been military; so that no alteration was made. The other battalion of the regiment joined in July. It arrived in perfect health, and encamped likewise on open ground; but still nearer the river. In a fortnight the intermitting fever began to make its appearance; and in less than three weeks, more than two thirds of the men were ill; whilst scarcely one of the officers had escaped. The officers, it must be remarked, encamped in the rear of the men, and immediately on the bank of the river, the course of which was uncommonly slow at this place; while its banks, though high, were oozy and foul. There are few instances on record perhaps, where a degree of sickness, greater than the present, has been observed in so short a space of time. The first battalion, however, did not suffer in the same proportion. The ground

of encampment was not only at a greater distance from the river; but being also nearer to a wood, many of those, who were not confined by their duty to a particular spot, found a convenient shelter in its shade, from the powerful heat of the sun. These I must not omit to mention, were the least sickly of the whole encampment. The above is an important fact. It proves clearly, that no ideal circulation of air can counterbalance the noxious exhalations from rivers; and it likewise affords a presumption, that instead of danger, there is safety in the shelter of wood. But with regard to this, no absolute rule can be given. It must generally be decided by local circumstances, whether wood, or open ground are to be preferred for the encampment of troops. Upon the whole, however, there are many reasons to induce us to believe, that as an encampment is not only more military in the body of a wood, than in open ground surrounded by woods; so it is likewise more safe with respect to health; particularly if within the reach of effluvia from swamps or rivers. The reason which offers is obvious. The wood not only stops the progress of noxious vapours carried from a distance; but it also covers the earth from the immediate action of the sun—the powerful cause of exhalation; in doing which, it perhaps, does more than counterbalance the less free circulation of air, or the greater dampness of the ground. But lest the authority I have mentioned, should not be thought sufficient, the opinion receives farther confirmation from the testimony of the ancients. Histories abound with examples of destructive epidemics, which have followed the cutting down of groves, which covered morasses, or which intercepted the progress of marsh exhalation. America also furnishes daily instances of a similar truth. In this country the unhealthiness of a place is often obviously increased, by cutting down the woods of the neighbouring swamps: hence no

rule is more liable to exceptions, than that which has been ſo generally enforced; viz. that clearing a country of its woods invariably renders it healthy: unleſs the grounds be drained and cultivated, as well as cleared, the effect is likely to be the reverſe.

It would be curious and uſeful, could we trace this miaſma or cauſe of fever in its progreſs. I do not deny that the noxious exhalation may be accidentally enveloped in fogs; but it is not neceſſarily ſo; and I add, that the dews of night, unleſs as an exciting cauſe, are leſs pernicious than has generally been imagined. Low grounds, in the ſame manner, are not always unhealthy; as high and dry ſituations ſometimes afford no protection againſt the ravages of this diſeaſe. The ſituation of the encampment which the 71ſt regiment occupied at King's-bridge, in the year 1778, affords a curious and direct proof of the truth of this opinion. About two hundred paces to the right of the ſpot, on which the tents were pitched, was a tract of low and ſwampy ground; but the immediate ſituation was dry, and of conſiderable elevation. The right was *particularly* ſo; yet it was principally on the right, where the diſeaſe raged with violence. The left, though on low ground, over which fogs frequently hung till late in the day, ſuffered in a much ſmaller proportion. From this we might infer, that a dry and elevated ſituation is by no means exempted from intermitting and remitting fevers: but the great degree of ſickneſs, which happened to thoſe people, who not being confined by the nature of their duty to one particular ſpot, pitched their tents on a hill in the rear of the encampment, proves it clearly. The ground, which thoſe perſons made choice of was directly in the tract of air, which blew over the ſwamp. It was dry and ſcarcely ever covered with fogs; yet there was not an individual among them who encamped upon it, who did not ſuffer from this raging epidemic. The preſent in-

ſtance, with many others which I might adduce, leaves little room to doubt, that inſtead of expoſing encampments to ſtreams of air, which blow from rivers or ſwamps, it ought to be our principal buſineſs to guard againſt thoſe noxious effluvia, by the interpoſition of woods or riſing grounds. Exhalations which are the cauſes of fevers are ſubtile, and ſeem to be pernicious, chiefly in their aſcent:—viſible damps or night dews are comparatively innocent.

So great is the importance of preſerving the health of an army in the field, that the choice of encampments ought to be made a ſubject of particular enquiry. The opinion of Sir John Pringle on this head, (which, in fact, is an opinion of theory rather than obſervation), has been followed too long without examination. The directions of this author are influenced wholly by the dread he entertained of a contagious or hoſpital fever; but a contagious fever, is ſeldom a diſeaſe of the field; and has, perhaps, ſcarcely ever been known to make its appearance in a moving camp. Diſeaſes of the field are often epidemic, ſometimes malignant, but rarely contagious. I even doubt if the dyſentery, whilſt a camp-diſeaſe, is ſo in any remarkable degree. It was not ſo at leaſt in America, in thoſe campaigns, where I had the opportunity of knowing the ſtate of the army.

The general remote cauſe of intermitting and remitting fevers, conſiſts, as was mentioned before, in inviſible exhalations floating in the air. Theſe are more copious in ſome ſituations than in others; and appear to be rendered more or leſs active by a great variety of cauſes. Among the number of thoſe cauſes which have been accuſed of exciting fever, it has been uſual to reckon exceſs in drinking. It cannot be denied, that this cauſe, in ſeveral caſes, has brought forth the diſeaſe, when it probably would not have otherwiſe appeared; yet it has been likewiſe obſerved that a debauch of wine has ſometimes reſtored the

body to health, when languiſhing under the influence of this diſorder in an obſcure or irregular form. The moderate uſe of wine, however, has been generally recommended as a preſervative in times of great heat, and epidemic ſickneſs:—and under limitations it undoubtedly is of uſe. In a time of very preſſing calamity, the oracle of Delphi gave its ſanction to the preſcription, and hiſtory bears teſtimony to its ſucceſs. But beſides exceſs in drinking, cold and fatigue have likewiſe been conſidered among exciting cauſes of fever. In ſhort, whatever exhauſts or diminiſhes the activity of the powers of life, may be juſtly viewed in this light. Yet ſtill I muſt obſerve that neither cold, fatigue, nor any of the cauſes of this train, give occaſion to a proper intermitting or remitting fever, unleſs the prediſpoſition to the diſeaſe be particularly ſtrong. As a proof of this, I muſt beg leave to mention a fact, which fell under my own obſervation. In an expedition into South Carolina, in the year 1779, a part of the army was near five hours in paſſing Puriſburg ſwamp. The men were always up to the middle, ſometimes up to the neck in water. The cold and fatigue were both very great, and a fit of intermitting fever was the conſequence in a great number of the ſoldiers: yet it was only in a few inſtances that the diſeaſe went through a regular courſe, though there was even a general pre-diſpoſition to it, in the habits of almoſt all the men who compoſed the detachment. The moſt of them had ſuffered from it ſeverely the preceding autumn; and a temporary return of it, was generally obſerved to follow any extraordinary exertion, or the application of a debilitating cauſe. The above cauſes are generally reckoned exciting cauſes of fever; but beſides theſe there are ſeveral others of conſiderable power, which as being commonly known, I ſhall not now ſpend time in enumerating. There however ſtill remains one, which, though

very univerſal, and perhaps more powerful than any other, has hitherto been little attended to. The approach to the new and full moon, in ſome degree, perhaps in every part of the globe, but particularly in the Weſt-Indies, appears to be connected with the invaſion and relapſe of fevers, in a very remarkable manner. This obſervation has been hinted obſcurely by one or two authors; the idea has been treated with ridicule by others: and it muſt be confeſſed, that the facts, which have hitherto been produced in ſupport of the opinion, are extremely vague and equivocal. I ſhall therefore enter a little more minutely into the ſubject, and ſtate circumſtantially the evidence, from which I have been led to conſider the approach to new and full moon, as a powerful exciting cauſe of fever.

That the moon exerts ſome influence on the human frame, and that her different appearances are more or leſs connected with the progreſs and iſſue of diſeaſes, does not ſeem to have altogether eſcaped the notice of the ancients. In a fragment of Hippocrates, in the edition of Vander Linden, we find a detail of the different aſpects of the moon and planets, with their combined influence on the fate of diſeaſes; but the ſtyle and manner of this little tract are ſo perplexed, that I do not pretend to underſtand its meaning. Galen had likewiſe ſome obſcure ideas on the ſubject; but he has left us nothing clear and explicit. The Arabian writers are alſo confuſed and inaccurate, ſo that the firſt circumſtantial evidence of the influence, or connexion of the moon with the human body, is found in the works of Ballonius, a French phyſician of the ſixteenth century. The fact which this author records, though not altogether in point, is curious. A Pariſian lady of quality appears by the account of Ballonius, to have been very ſingularly affected during an eclipſe of the ſun. Her complaint threatened nothing dangerous, and her phyſicians

were amusing themselves with observing the progress of the eclipse, when they were suddenly summoned to her assistance. In the moment when the eclipse was deepest, she had the appearance of dying; but these threatening symptoms decreased with the decrease of the eclipse; so that she at last returned to her former state. This is only a solitary instance, and perhaps might be reckoned accidental. We may however add to it the general testimony of Ramazzini, who lived at Modena in the beginning of the present century. This author's observations, indeed, are by no means precise; yet he was convinced by them, that the course of Epidemics was considerably influenced by the particular state of the moon. It is almost needless to mention Dr. Mead, who wrote a treatise expressly on the moon's power on the human body. The facts which this writer has collected, afford a reasonable presumption, that this planet is not without some influence in several diseases to which man is liable; but we find not any thing in the work, which particularly relates to fevers. I shall mention a fact recorded by Dr. Grainger. It is the most circumstantial I have yet met with; and the strongest to be found perhaps in the writings of any European physician. Dr. Grainger, who was a surgeon of the army, served in the Netherlands about the years 1746 and 47, and wrote a treatise on the intermitting fevers of that country. Among other observations he takes notice of a circumstance which occurred to him at that time, and which he then considered as singularly curious; viz. that twenty of the men of the regiment, of which he had the charge, were seized with this fever, which was then epidemic, on the day of a solar eclipse. He has not made any application of the fact. It furnishes however a very substantial evidence, of the influence or connexion of this planet with the invasion of febrile diseases.

It appears to have been long known in India, that

fevers have a tendency to relapſe about the new and full moon, and particularly at the time of eclipſes; but Dr. Lind of Windſor is the firſt, who brought the knowledge of the fact to Europe. In an inaugural diſſertation, publiſhed at Edinburgh (I do not exactly recollect the year), this author obſerves, that this opinion prevailed very generally in the Eaſt. He adds likewiſe, that ſome inſtances occurred in his own practice, which gave him cauſe to believe that the fact was well founded. Dr. Lind continued of this way of thinking for ſeveral years after his return to England. He does not indeed at preſent deny the fact. He only ſuggeſts that it may admit of a different explanation, from that which he had given in his firſt publication. The ſpring tides, as they overflow the low grounds, according to his preſent opinion, afford a more probable cauſe of the uncommon increaſe of fevers about the new and full moon, than the direct influence of the planet itſelf. I will take the liberty however to add, that this opinion has been offered to the public, from a very imperfect view of the ſubject. I can affirm, even from the confined circle of my own experience, that a connexion, between the moon and the invaſion of fevers, certainly takes place in diſtricts remote from the ſea; and I believe it is generally known, that a fever, or the paroxyſm of fever, is not commonly the inſtantaneous conſequence of expoſure to its remote cauſe; which ought to be the caſe, if this author's reaſoning were juſt.

The next, and indeed the only author who has written profeſſedly on the influence of the moon in fevers, is Dr. Balfour; a gentleman who reſided ſeveral years in India, and who practiſed with reputation in the ſervice of the Company. This author pretends to have inveſtigated the ſubject with care and attention; but there appears in reality, to be more theory and general aſſertion in the treatiſe than circumſtantial

fact. The result of his observations he informs us, amounts to this: viz. that the three days which precede, and the three days which follow new and full moon, are remarkable for the invasion and relapse of fevers; that the day of the full moon, and the day of the change of the moon, are the most remarkable of all: and farther, that the days which follow, are, in general, more remarkable than those which precede.

I have now brought together the substance of what is found in the writings of those authors who have mentioned cursorily, or treated professedly of this subject. There is not in any part of it, if we except the instance recorded by Dr. Grainger, any thing accurate and precise enough to enable us to form an opinion. What has fallen under my own observation, I would flatter myself, is less ambiguous; and though it may not be so explicit, perhaps, as to establish the doctrine completely, it may at least assist us, I hope, in approaching nearer to the truth.—I shall relate it in a few words.

When I arrived in Jamaica, in the year 1774, I had no other knowledge of the influence of the moon in fevers, than what I retained from a cursory reading of Dr. Lind's dissertation. I remember, however, to have mentioned the circumstance to several practitioners, who had lived many years in the island. As I conceived there was a similarity between the climates of Jamiaca and Bengal, I thought it not improbable, that some of the practitioners of the country in which I then was, might supply me with satisfactory information on the subject. There were none of them, however, who acknowledged that they had ever observed any connection between the moon and febrile diseases; neither were there many of them, who seemed disposed to give credit to its existence. Twelve months or more elapsed without my having paid any further regard to the fact, when an acciden-

tal relapſe of fever, happening near the time of full moon, recalled Dr. Lind's obſervation to my memory. It likewiſe brought to mind a circumſtance, which till then I had overlooked. I had ſeen frequently, though without attending to it particularly, that three or four of the ſoldiers of a company of the 60th regiment, who were quartered at Savanna la Mar, and of whom I had the care, were attacked with fever on the ſame day; whilſt it ſeldom happened, that any other febrile illneſs made its appearance in the garriſon, for the enſuing fortnight. This having been obſerved oftener than once, at the time the moon was near full, a hint ſuggeſted itſelf, that the cauſe, which was ſaid to influence relapſes in India, might here have an effect on the original invaſion. But in order to aſcertain the truth of this conjecture, which I conſidered as a matter of ſome importance, I provided myſelf with the almanack of the year 1776, and marked, in the blank leaf of it, the preciſe date of attack, of all thoſe fevers which came under my care. In looking over thoſe memoranda at the end of the year, I found I had put down thirty caſes of proper remitting fever, the invaſion of twenty-eight of which was on one or other of the ſeven days, immediately preceding new or full moon; that is in the ſecond and laſt quarters. The ſame plan of obſervation was continued through the following year, and the reſult, though not exactly the ſame, was ſimilar. Of twenty-eight caſes, which where found in the almanack, twenty-two were in the periods above-mentioned: that is in the ſecond and laſt quarters of the moon. It deſerves however to be remarked, that three of thoſe ſix caſes, which were not in the common period of invaſion, happened actually on the day of new moon;—a few hours after the change had taken place. But beſides thoſe caſes of proper remitting fever which I have mentioned,

there were likewiſe found in the almanack many days fevers and ſlight feveriſh diſorders, the invaſion of the greateſt number of which was likewiſe in the uſual period.

The above is a literal ſtate of the caſe as it ſtood in the almanack:—ſome remarks and obſervations, however, were added, of which the following are the principal: viz. That, though the whole of the ſecond and laſt quarters of the moon is included in this period of invaſion; yet the four days immediately preceding new and full moon, were more particularly diſtinguiſhed for thoſe febrile attacks: that in the dry ſeaſon, which is reckoned the moſt healthy, the time of invaſion was more cloſely connected with the new and full moon, than in the wet and ſickly months, particularly when the ſickneſs was epidemic, or of a bad kind: and laſtly, that this influence, or connexion was more apparent in the ſoldiers of the garriſon, who were expoſed to few occaſions of diſeaſe, exceſs in drinking excepted, than in the inhabitants of the town and country, whoſe occupations carried them oftner to places of unhealthy ſituation; or whoſe modes of life obliged them to ſubmit to more various hardſhips or to greater fatigues than fell to the lot of a ſoldier in times of peace.

I ſhall further beg leave to add, that I went to join the army in America, in the year 1778; and that I continued in that country, the train of obſervation on this ſubject, which I had begun in the Weſt Indies. The regiment, in which I ſerved, was encamped during the months of June and July on a healthy part of York-iſland. Fevers were rare; and the time of invaſion, of ſuch as did appear, was chiefly confined to the ſecond and laſt quarters of the moon. In the beginning of Auguſt, the encampment was removed to King's-bridge, where it occupied a very unhealthy ſituation. The intermitting fever ſoon

made its appearance. It extended in some degree to the whole battalion; but raged with particular violence on the right, which bordered on low and swampy ground. The approach to new and full moon never failed, even in this climate, to increase the number of the sick; yet it deserves to be remarked, that this increase was always smaller in proportion, in that part of the battalion, which lay contiguous to the swamp, where the disease was highly epidemic, than in the other extremity of the encampment, where it prevailed in a less degree. But still upon the whole, when the regiment moved from their ground, in the beginning of November, of a hundred cases of intermitting fever, which were marked in the almanack, eighty were found to have commenced in the usual period of invasion; that is, in the second and last quarters of the moon. It is somewhat remarkable, that relapses were in a smaller proportion. This regiment, some parts of the medical history of which I describe, embarked on an expedition for the southward in November, and arrived at its destination in Georgia, in the latter end of the year. It remained in the southern provinces, and served every campaign till the capitulation at York-town. The same train of observation was continued during this intervening space, and the same influence of the moon seemed in general to prevail; but the notes having been lost, I cannot now exactly ascertain the degree in which this influence took place. Of this, however, I am certain, that even in times of the greatest epidemic sickness, when the connexion was evidently weakest, the number of the sick was generally doubled in the periods approaching to new or full moon.

We cannot avoid concluding, from the facts which I have stated above, that the approach to new and full moon, or something connected with that approach, may be justly considered as a powerful exciting cause

of fever. The circumſtances, indeed, which I have mentioned, are ſo clear and unequivocal as to leave little room for doubt: nor did I entertain any, till I found that the obſervations of Dr. Balfour, on this ſubject, were ſo ſtrikingly different from mine. Bengal and Jamaica are diſtant from each other; yet few people will be diſpoſed to believe, that ſo great modification of a general cauſe has ariſen ſolely from this diverſity of climate. Dr. Balfour muſt ſpeak for himſelf. For my own part, I can only ſay, that what I ſaw I have related with truth. As I have told the manner in which the idea aroſe, with the manner in which the inveſtigation was conducted, I leave the concluſion to be formed by the reader.

CHAP. V.

THE PROXIMATE CAUSE OF FEVER.

THEORIES of the proximate cauſe of fevers, or more properly modifications of theories, are ſo numerous, that a whole volume would ſcarcely be ſufficient to give any tolerable account of them. It is a taſk indeed which I ſhall not undertake; yet I hope it will not be altogether ſuperfluous, to give a curſory view of the principles, which have directed the conjectures on this ſubject in different ages. The principles are, in fact, fewer in number than at firſt ſight they appear to be. Phyſicians, ambitious of raiſing their name and reputation, have ſhown great induſtry in multiplying and modifying opinions; yet it does not appear, that they have produced any great variety of theories, which are fundamentally diſtinct.

The ancients, who were little acquainted with chemical principles, or with the qualities and properties of the nervous ſyſtem, placed the proximate cauſe of fevers in ſome ſignal ſymptom of the diſeaſe, ſuch as increaſed heat, or abounding bile; or entering ſtill farther into the fields of ſpeculation, ventured to attribute it to derangements in the permeable canals of the body, or to affections of the humours, or circulating maſs of fluids. Hence obſtruction of pores, plethora, error loci, lentor and viſcidity, or putrefaction of the humours, have all ſeverally, at different times, or by different authors, been conſidered as the immediate or proximate cauſes of this diſeaſe. The theories, which prevailed in the ſchools till the beginning of the ſixteenth century, did not often extend farther than to the cauſes which I have mentioned: but after that period, the diſcoveries of

the famous Paracelsus opened a road to innovation in medical reasoning. The followers of this author, if not numerous, were enthusiastic and vociferous. They indulged in the wildest extravagance of conjecture; and their opinions, for a considerable time, were combated with the authority of Galen, rather than with solid argument and accurate reasoning. At last the disputes between Chemists and Galenists beginning to subside, the chemical theories became incorporated with the doctrines of the mechanic philosophy, which were revived more than a century ago, and which still maintain some influence in the common systems of physic. In the mean time happened the important discovery of the circulation of the blood; but no immediate change, in the manner of accounting for fevers, ensued immediately in consequence of it. Yet as from this period the researches of physicians began to be conducted on a more extended plan, some parts of the system were brought into view, which had been formerly little attended to. The nervous system, which had been in a manner overlooked for many ages, was now found to be of importance in the economy of the animal machine; and authors soon began to consider it, as affording a probable seat for the proximate cause of fevers. Among the first of those authors, who viewed it in this light, we reckon Borelli and Dr. Cole; the one of whom in Italy, the other in England, proposed much about the same time, new and different opinions about the proximate cause of fevers. Their conjectures, I must confess, are far from being probable;—(that of the Italian is scarcely ingenious) yet they deserve to be mentioned in this place, as being among the first attempts to bring into view a part of the system, which is very essential in enabling us to account for many appearances in febrile diseases. It is commonly believed, that the nervous system was not discovered to be a part of material import-

ance; either in the functions of health, or in the affections of ſickneſs, till the laſt century. This, in fact was generally the caſe; yet I muſt not omit to mention, that we meet with an expreſſion in the writings of Hippocrates, viz. τα ορμωντα η ενορμωντα σωματα, which might incline us to be of opinion that this phyſician was not altogether ignorant of the influence of the nervous power; and that he actually conſidered this principle of the conſtitution to be of much importance in the management and cure of diſeaſes. After Hippocrates, Van Helmont, under the whimſical appellation of Archeus, aſſerted more directly the dominion of the ſentient principle. He has indeed applied its operations more particularly to aſſiſt him in explaining the theory of fevers; but it has been a misfortune that the opinions of this author have been generally leſs attended to, than perhaps they deſerve: ſo that it has been cuſtomary to conſider, the celebrated Hoffman as the firſt, who ſuggeſted the idea, that the proximate cauſe of fever depends on a derangement or affection of the nervous ſyſtem; at leaſt he is the firſt, who delivered a ſyſtem on the ſubject, which can in any degree be conſidered as rational and conſiſtent.

It will not be an eaſy taſk, to give a clear and diſtinct view of that, which has been conſidered by the ancients, as the proximate or immediate cauſe of fevers. The language of the earlieſt writers is not by any means preciſe in this reſpect; and we ſhall frequently, perhaps, have difficulty from the ambiguity of expreſſion, to diſtinguiſh from each other the definition, the remote and occaſional, or the immediate and proximate cauſe of the diſeaſe. The proximate cauſe of a diſeaſe, it muſt be remembered, is a cauſe which conſtantly and uniformly produces its reſpective complaint; and without which this complaint cannot even for a moment exiſt. It is, in ſhort, the firſt eſſential derangement, which the ac-

tion of this cauſe produces in the frame of the ſufferer: but though we know this to be certainly true, yet we have made no progreſs in diſcovering the nature of this derangement. The firſt action of the cauſe of fever is obſcure, and ſome part of the derangement which it occaſions, has hitherto probably paſſed over unnoticed, even by the moſt accurate obſervers.

If we attempt to give a view of the ſucceſſive conjectures, which, at different times, have been offered to the public concerning the proximate cauſe of fevers, it will be neceſſary to begin with Hippocrates. We may collect very clearly from the writings of this author, that an increaſe of the heat of the body had afforded, to the ſtill more ancient phyſicians, the firſt idea of the eſſence or immediate cauſe of fevers. This ſeems to have been the idea of the moſt ancient profeſſors of medicine. Hippocrates in ſome degree ſubſcribed to it; yet this author ſeems likewiſe to doubt, if the ſimple increaſe of heat alone is ſufficient to conſtitute a proper fever, or that it can with propriety be conſidered as the eſſential proximate cauſe of the diſeaſe. But though Hippocrates raiſes this objection to the common opinion concerning heat, yet he ſtill leaves us in doubt with regard to the opinion which we ought to adopt. His ideas are fluctuating and uncertain. We find in the different parts of his works, obſtruction, plethora, miaſmata or bile, all ſeparately conſidered, as immediate cauſes of fever. But ſuch cauſes, I may add, where they do take place, are in fact only more remote or diſtant cauſes. Neither miaſmata, bile, nor obſtruction, are circumſtances on which the exiſtence of fever invariably and neceſſarily depends; at leaſt ſuch cauſes require to be in a certain ſtate of modification, which is yet undefined, before they are capable of actually producing the diſeaſe. Bile bears a very conſpicuous part in the Hippocratic

doctrine of fevers. The fabric, indeed, which our author raises on this principle, is fanciful, and in many respects, ill founded; yet, as modified by the fertile genius of Galen, it passed on through a succession of many ages: nor is it, even now, altogether banished from the language of practitioners.

Such are the hints concerning the causes of fevers, which I have been able to collect from the writings of Hippocrates. The expressions are often obscure or equivocal; and we can scarcely say, that an opinion can be formed from them which deserves the name of a theory. The successors of this great physician were, perhaps, too sensible of this defect; and therefore attempted to fabricate other opinions, which might be more explicit and distinct. Among the first of those attempts, we may reckon the hypothesis of Diocles of Carystus, a physician who lived at an early period, and who was highly esteemed by his contemporaries. Fever, according to this author, is not so much a primary disease, as a symptom of some other affection. Wounds, tumours, and many other accidental causes, have certainly been observed to give rise to symptoms which have been usually denominated fever; yet neither wounds nor inflammations have been generally observed to give rise to a proper fever. I will not however deny, that wounds, or inflammations, occasionally prove exciting causes of proper fever, where there is a strong disposition to the disease, existing in the constitution, at the time those accidents have happened. It does not appear that this theory of Diocles gained much ground with succeeding writers; yet it was, perhaps, the cause of introducing the distinction of primary and symptomatic into the history of fevers; a distinction, which is frequently of consequence in practice. But I must further add, that though the opinion of Diocles is not admissible in its literal meaning; yet, in a modified sense, it is not altogether without foundation.

The ſymptoms of fevers are undoubtedly indications of a derangement of the body from its healthy ſtate; but when we have ſaid this, we can ſay no more.—The nature of the derangement, which in its firſt beginnings is not obvious to the ſenſes, neither the ancients, nor the writers of the preſent age have, as yet, been able to aſcertain.

Not very long after Diocles, Eraſiſtratus, a native of the iſland of Cea, and phyſician at the court of Antigonus, furniſhed a conjecture concerning the cauſe of fevers, which is mentioned both by Celſus and Galen, and which appears to have originated in his anatomical reſearches. As Eraſiſtratus directed his purſuits particularly to the ſanguiferous ſyſtem: ſo impreſſed, perhaps, with an idea of the importance of that part of the body on which his thoughts had been chiefly employed, he ventures to hazard the opinion, That the immediate cauſe of fever depends on a certain error loci, or tranſfuſion of the red blood into the arterial channels: and this, he moreover adds, proceeds from repletion.—The opinion originates from an anatomical error, and on that account need not detain us any longer.

The next author, of whoſe opinion on this ſubject any diſtinct traces have been tranſmitted to us, is Aſclepiades, the Bythinian, a man who ſeldom treats the doctrines of his predeceſſors with reſpect. In his rage for innovation, Aſclepiades attempted to change or modify the theories of thoſe who had gone before him, in ſuch manner, as to hope to impoſe a conjecture on the world, which might, at leaſt poſſeſs ſome exterior claims of novelty. He allows with the moſt ancient phyſicians, that the inſeparable ſign of fever, or its eſſential part, conſiſts in an exceſs of heat; but having adopted the doctrine of atoms, which was conveyed to the Greeks by Democritus of Abdera, he pretends to account for the difference of types by a difference in the ſize of the corpuſcles, which he

ſuppoſes to be formed by a combination of indiviſible atoms. Thus we ſee that obſtruction in the permeable canals of the body, in this writer's opinion, conſtitutes the theory of the proximate cauſe of fever: on which principle we may likewiſe conclude, that the modern doctrine of lentor and viſcidity has built its foundation.

The author, whom I have laſt mentioned, may actually be conſidered as the original founder of the methodic ſect. The principal tenets of this ſect of phyſicians have been tranſmitted to us by Celſus, Cælius Aurelianus, or Galen; but the doctrines, which they promulgated, have not been very fully and perfectly explained. The great diviſion of Themiſon, into ſtrictum et laxum, furniſhes a very ſimple view of diſeaſes. Fevers are included in the firſt order of derangement; and in this reſpect, may be conſidered as depending on a cauſe ſimilar to the obſtruction obſcurely hinted by Hippocrates, or more explicitly deſcribed by Aſclepiades. There is this difference, however, between theſe reſpective opinions, that the earlieſt writers ſeem to have referred the obſtruction to ſome change in the humours or circulating maſs; while the *methodics* appear to have attributed it more directly, to a change in the capacity of the containing veſſels. Hence we may infer, without any improper latitude of interpretation, that the ſtrictum of Themiſon and Theſſalus comprehends the ſpaſmodic conſtruction of capillaries, which has lately made ſo conſpicuous a part in the theory of febrile diſeaſes. This theory of the methodics, where the nervous and fibrous ſyſtem have been more regarded than the humours, or circulating maſs of fluids, was principally followed at Rome, for more than a hundred years. At laſt Galen, who was a very unqualified admirer of Hippocrates, exerted himſelf ſo ſucceſsfully in reviving the humoral doctrine of his maſter, that the methodic ſect began to ſink rapidly into

decay; and after a ſhort time its traces were totally obliterated.

The frequent blanks, in medical hiſtory, make it no eaſy taſk to give a connected view of the fluctuating ſyſtems of the ancient phyſicians. The works of every writer of the methodic ſect have periſhed, except thoſe of Cælius Aurelianus: neither have we been able to diſcover any new opinion, or modification of opinion, concerning the proximate cauſe of fevers, between the time of Aſclepiades or Themiſon, and the great commentator of Hippocrates, except that of Athenæus. Athenæus, who was the head of the ſect of Pneumatics, ſtood high in eſteem among his contemporaries and ſucceſſors. This author ventured to ſuggeſt a new hypotheſis, or more properly perhaps, only extended, and more fully explained a doctrine, of which the obſcure traces may be diſcovered at an earlier date. The general cauſe of fever, in this writer's opinion, conſiſts in a putrefaction, or putreſcent ſtate of the humours. Hippocrates ſeems to have entertained ſome indiſtinct idea of the ſame kind; and thoſe, who have been inclined to this way of thinking, both in ancient and in modern times, have neither been few in numbers, nor contemptible in authority.

Galen, who has written on moſt parts of medical ſcience more learnedly than his predeceſſors, has diſcuſſed very fully the ſubject of the proximate cauſe of fevers. Amidſt the luxuriance of this author's colouring, it is ſometimes difficult to lay hold of the preciſe idea; at the ſame time, that it is oftener tedious than inſtructing to follow him through the maze of his fanciful and inconcluſive reaſonings. I ſhall not therefore enter into a minute detail of his arguments; but ſtill I conceive it may be uſeful, particularly to thoſe who have not the opportunity of conſulting his voluminous, and in ſome reſpects ill digeſted works, if I compreſs into narrow compaſs the lead-

ing principles of his general doctrines. In the firſt place, the opinion, hinted by Hippocrates and adopted by moſt of his ſucceſſors, that the eſſence of fever conſiſts in a certain derangement of heat, is expreſsly maintained by Galen, who explains more elaborately than his predeceſſors the various circumſtances, which influence or modify this general cauſe of the diſeaſe. Galen aſſumes, indeed, as a fundamental poſition, that heat any how, or any where excited, communicated to the heart, and from the heart to the reſt of the body, conſtitutes a fever; yet he afterwards adds more explicitly, that a preternatural heat does not conſtitute a fever, unleſs it is communicated to the heart; which is conſequently to be conſidered as the principal ſeat and reſidence of the febrile affection. Having, as he imagines, eſtabliſhed this fundamental principle, he proceeds to inveſtigate, more particularly, the parts of the body where the heat reſides, and the cauſes by which it is generated, propagated, or ſo modified, as to produce the diſeaſe in its different forms. But, that he may the better explain his meaning clearly, he divides fevers into three different kinds: viz. the hectic, or habitual, the humoural, and the ephemeral. The firſt he ſuppoſes to ariſe from an affection of the ſolids, or containing parts; the ſecond from ſome derangement of the fluids, or contained parts; and the third from ſome diſturbance of the ſpirits, or that part of the frame which we, perhaps, now diſtinguiſh by the name of nervous ſyſtem. He adds in the next place, that putrefaction is the medium, by which fever is excited, where the fluids or humours are the ſubject of the diſeaſe, contiguity and continuity, where the illneſs affects the habit or ſolid parts; and where the effects are tranſitory aud fleeting, he attributes the cauſe principally to the rapid movements of the ſpirits, or nervous influence. And laſtly, he attempts to complete his theory, by explaining the different types of humoural

fevers, on the supposition of a state of putrefaction in the different humours, from which he supposes the disease to arise. On this subject he has deviated very materially from his master Hippocrates, though he probably drew his ideas from the hints, which are found in that author's works. Hippocrates explains, or attempts to explain the various types of fevers, by a simple difference in the quantity of the bile. Galen, on the contrary, as we have said just now, endeavours to account for this phenomenon, by a supposition of putrefaction in the phlegmatic and bilious humours, which bear so conspicuous a part in his theoretical system. Thus Galen supposes, that a putrescent tendency in the blood gives rise to a continued fever; a similar disposition in the phlegm disposes the disease to appear in a quotidian form: putrefaction of the yellow bile determines the type to be of the tertian kind; whilst a like tendency, in the black bile, regulates the movements of the quartan period.—It is unnecessary to make any remarks on the baseless fabric, which this author has offered to the world, concerning the proximate cause of fevers.—Its inconsistency and insufficiency are perfectly obvious.

After the time of Galen there does not appear to have been any material change, in the manner of accounting for fevers, for many ages. Aetius Amidenus indeed suggested some restrictions and explanations in certain species of fever, which do not seem to have been so explicitly marked by the commentator of Hippocrates. Instead of considering putrefaction as the sole means of exciting heat in every species of humoural fever, Aetius ventures to insinuate, that there is no state of actual putrefaction in the συνοχος, or that species of disease which is purely inflammatory, the cause of which appears to be simply an inordinate fermentation or ebullition of the blood. But except in this instance, the succeeding Greek physicians do not seem to have departed, in the least, from

the direct footsteps of Galen. The Arabians likewise, among the principal of whom we may reckon Avicenna, adopted his general doctrines, and modes of reasoning, only Avicenna defines more expressly than others had done before him, that fevers of all denominations arise immediately from a preternatural heat of the heart; in doing which, he seems to have extended the influence and power of that quality which preceding authors in looser terms had considered as the general cause of febrile diseases.

The doctrines of Galen, with some immaterial innovations of the Arabian physicians, wholly occupied the schools of medicine, till the beginning of the sixteenth century, about which time Aureolus Philippus Theophrastus, commonly known by the name of Paracelsus, effected a revolution of opinions, which marks an important period in the history of the medical art. Paracelsus, who was a man of a singular turn of mind, spent the earlier part of his life in travelling among the nations of Asia; where he probably acquired some knowledge of chemistry, in which science the Arabians appear, even at that time, to have made considerable progress. The knowledge, which Paracelsus carried home to his native country, was not generally known in Europe. This author applied it with success in the cure of some desperate diseases; and acquired uncommon fame from his new and unheard-of remedies. He was an empiric in the theory, no less than in the practice of the art; and I may add, that his attempts to overturn the doctrines of the ancients, give an indication of more effrontery than genius or knowledge. The wonderful cures of obstinate diseases, which he was said to perform; and still more, perhaps, the mysteriousness of his language, which caught the notice of the vulgar, who often imagine that knowledge is concealed under terms, which they do not understand, brought followers to his standard. Thee-

ries of the proximate cauſe of fevers, were fabricated without difficulty, by the help of thoſe principles, which Paracelſus had introduced to the acquaintance of the world; yet it does not appear, that any theory aroſe, during this period, which had probability, or even ingenuity for its ſupport. The period indeed, during which chemical reaſonings ſo univerſally prevailed, may be ſtyled juſtly enough a period of medical romance: and I ſhould conſider it as a treſpaſs on the patience and good ſenſe of the reader, to ſpend time in refuting the abſurd and incongruous doctrines of ſulphur, nitre or mercury; acid and alkali, or the various modes of fermentations, which for a time filled the writings of phyſicians. The mechanical mode of reaſoning, which ſucceeded, or rather which became incorporated with the doctrines of the chemiſts, ſeemed at firſt to promiſe greater advantages: but though theories of fevers were formed by many eminent men, both of the laſt and preſent century, on the principles of the mechanical or chemico-mechanical philoſophy; yet there are not any of them, which ſeem to have afforded a ſatisfactory explanation of the ſubject.—The ſo-much celebrated doctrine of lentor and viſcidity was aſſumed without evidence of its exiſtence, and perſiſted in, without being ſufficient to account for the phenomena of the diſeaſe.

Thoſe conjectures concerning the proximate cauſe of fevers, which I have mentioned hitherto, can ſeldom be ſaid to extend farther than to a particular ſtate of the humours, or circulating maſs of fluids, which, according to the prevailing philoſophy of different ages, have been ſuppoſed to be changed from their natural and healthy ſtate, by chemical or mechanical derangement. I obſerved before, that it might appear, from an accidental expreſſion in the writings of Hippocrates, that this author was not altogether ignorant of the influence or effects of a nervous power, or ſentient principle. The methodic

ſect likewiſe, may ſeem to have comprehended in the idea which they have given of diſeaſes, that there is ſome derangement of the fibrous ſyſtem; or perhaps that a ſpaſmodic ſtricture of capillaries is actually the immediate cauſe of fever; whilſt Galen every where celebrates the powers of nature or vires naturæ medicatrices, which bear in his opinion, a very active part in the cure of febrile diſeaſes. To thoſe vague ideas of the ancients, we may add the more modern and explicit doctrine of Van Helmont, who was a man of genius, learning and obſervation. Van Helmont adopted the ſentient principle of Hippocrates; but he alſo applied it in a bolder light than had been done by its original author, and employed its aſſertions more particularly towards the explanation of the cauſe and phenomena of fevers. The enthuſiaſm of this writer diſguſts the philoſophic ſpirit of the preſent age, and we muſt acknowledge, that his ideas are often unphiloſophical and abſurd; yet we muſt likewiſe do him the juſtice to add, that the principle of his doctrine in ſome degree is well founded, and that his views, in many reſpects, are important in practice. I muſt further obſerve, that the efforts of nature, ſo celebrated by Campanella and Sydenham, and even, perhaps, the αυτοκρατεια, of Stahl and his followers, can only be conſidered as modifications of the furious Archeus.—But though the authors I have mentioned, ſeem evidently to have poſſeſſed ſome vague idea of the powers or influence of the nervous ſyſtem; yet there are not any of them, who have attempted to explain its operations by a philoſophical and conſiſtent mode of reaſoning. The period of this improvement is not very remote.

As ſoon as the circulation of the blood was known and fully eſtabliſhed, the heart loſt ſome part of its former importance; whilſt the brain and nerves, which for many ages had been little regarded, roſe into primary and eſſential conſequence. But

though the brain and nerves were discovered, soon after this period, to be the instruments of life and motion; yet the laws of this part of the system were at first only imperfectly understood; and the attempts to explain its operations were, for a while, whimsical and absurd. Willis deserves some credit, as being one of the first who brought the general importance of the nervous system into view: but Borelli, an Italian mathematician, actually appears to be the first who ventured to ascribe the proximate cause of fever, to a particular derangement of this part of the frame. The immediate cause of fever, in this author's opinion, depends on some unusual acrimony of the nervous fluid; but it is only necessary to observe with regard to this doctrine, that a supposition of acrimonious fluids, where a fluid cannot be proved to exist, is so perfectly visionary, as only to deserve to be mentioned, from its being the first attempt to bring this part into view, in accounting for febrile disease. This hypothesis, however, though obviously ill founded, enjoyed its day of fame. It was soon followed by another conjecture, more ingenious indeed, but which was not so generally attended to, as the preceding. Dr. Cole of Worcester, towards the end of last century, suggested an idea, that the proximate cause of intermitting fevers depends on a laxity or debility of the brain and origin of the nerves. The supposition is not so improbable; but the superstructure, which the author has raised, is absurd, and unsupported either by fact or probability. Yet, if we expect Mundy, an author who offered a conjecture of a similar kind, in a work entitled Βιοχρησολογια, Borelli and Cole are the only writers prior to the time of Hoffman, who considered the nervous system, as directly affording a seat for the proximate cause of fevers. Hoffman, whom I have just mentioned, was a celebrated professor at Halle in Saxony. He flourished in the earlier part of the present century,

publiſhed many volumes, and certainly poſſeſſes the merit of having enlarged our views on the ſubject of fevers. His theory of the proximate cauſe is not only more ingenious, but certainly has more appearance of truth, than any other, which had been offered to the public at the time it appeared. The cure of fever conſiſts, in his opinion, in a ſpaſmodic affection of the nervous ſyſtem. It is a truth which few people will attempt to deny, that a ſpaſmodic ſtricture of the ſurface of the body generally takes place in ordinary caſes of fever; yet we muſt perhaps alſo acknowledge with Dr. Cullen, that a ſpaſmodic ſtricture is not certainly and uniformly the firſt eſſential part of a febrile diſeaſe. Some other thing is frequently obſerved to precede the ſpaſm, which, in the opinion of the laſt mentioned celebrated profeſſor, has a right to be conſidered as a proximate and eſſential cauſe. But as the theory of the proximate cauſe, aſſigned by Dr. Cullen, is not only more plauſible and complete than any preceding one; but ſtill increaſing in popularity and fame, it will not be ſuperfluous, if we ſtop to examine it with more attention. The remote cauſes of fever, according to this author, are ſedative powers, applied to the nervous ſyſtem, which diminiſhing the energy of the brain, thereby produce a debility in the whole of the functions, and particularly in the action of the extreme veſſels. Such, however, is at the ſame time the nature of the animal economy, that this debility proves an indirect ſtimulus to the ſanguiferous ſyſtem; whence by the intervention of the cold ſtage and ſpaſm connected with it, the action of the heart and larger arteries is increaſed, and continues to be ſo, till it has had the effect of reſtoring the energy of the brain, of exciting this energy to the extreme veſſels, of reſtoring therefore their action; and thereby ſpecially overcoming the ſpaſm affecting them: upon the removing of which, the excretion of ſweat, and other marks of

relaxation of excretories take place. This theory of fever holds out an appearance of great ſimplicity, and of perfect connexion. I wiſh we could ſay that it had an equal claim to truth: but I am afraid it will be found, on a careful examination, to be no more in reality than an ingenious hypotheſis, the leading principles of which can ſcarcely be proved even to exiſt. I do not pretend to enter deeply into the diſcuſſion of the ſubject; yet I cannot avoid repreſenting, in a few words, ſome circumſtances of difficulty in this author's theory, which are not eaſily reconcileable, either with reaſon or obſervation. It might be doubted, in the firſt place, if the remote cauſes of fever are actually of a ſimple ſedative nature; but at preſent I ſhall admit that the firſt principle, which is aſſumed by the profeſſor, is in reality a fact, and proceed to enquire, if the reſt of the doctrine is capable of being defended, even on this foundation. It conſtitutes the ſum of Dr. Cullen's theory, as was mentioned before, that the remote cauſes of fever occaſion debility, or diminiſhed energy of the brain and nervous ſyſtem; that this debility neceſſarily gives riſe to ſpaſm, and increaſed action of the heart and arteries; which continuing for a certain length of time, finally removes the diſeaſe. Thus the different ſtages of fever appear to follow each other as cauſe and effect; and debility in the firſt inſtance, is ſuppoſed neceſſarily to give riſe to reaction. Such a ſuppoſition is not very obvious to reaſon, and has not much ſupport from the analogy of facts. It would be eaſy to mention examples, where the application of debilitating cauſes as is not obſerved to be followed by obvious reaction of the ſyſtem; but at preſent I ſhall content myſelf with the familiar one of the application of cold. It is perfectly well known, that cold, when conſtantly and uniformly applied to the body, even goes ſo far as abſolutely to extinguiſh the powers of life, in a part, or in the whole, without

our being able to perceive any efforts on the part of nature to ſtop the progreſs of this deſtructive tendency. From this we may fairly infer, that common debilitating cauſes, at leaſt while they continue to be applied in the ſame conſtant and uniform manner, do not neceſſarily excite the reaction of the ſyſtem: but I will even go farther, and venture to affirm, that ſpaſm and reaction do not neceſſarily follow very great degrees of debility, which appear to proceed from the preſence of a febrile cauſe. During the time I remained in America, I had frequent opportunities of witneſſing the truth of this aſſertion. In the ſouthern provinces of that country, particularly in the ſummer and autumnal months, the intermitting fever was generally epidemic in a high degree; but its general cauſe, which was then ſo abounding in the atmoſphere, often injured the actions of life, without producing a regular train of operation; that is, one part of the diſeaſe appeared without that mode of action, which is ſuppoſed, by our author, to be its neceſſary effect. Thus, I have ſeen the moſt extreme degrees of debility and langour in all the functions, continue even for eight or ten days, without our being able to diſcover the ſmalleſt marks of ſpaſm, or obvious reaction. This inactivity and langour ſometimes vaniſhed ſuddenly; and the body reſumed its ordinary health and vigour, frequently without an evident cauſe. On the next day, however, or perhaps the day following it, the patient was ſurprized with a regular paroxyſm of fever. From this it appears very plainly, that if the immediate cauſe of fever actually conſiſts in debility, this debility neceſſarily undergoes a peculiar, but hitherto undefined ſpecies of modification, before it can be conſidered as the cauſe of the ſubſequent parts of the diſeaſe; a conceſſion which leaves us perfectly in our former ſtate of uncertainty and ignorance.

As it may be concluded from the facts, which I

have mentioned, that ſpaſm and reaction are not the neceſſary conſequences of the application of debilitating cauſes, either common or febrile; ſo if we purſue our author's train of reaſoning farther, we ſhall not find his inductions to be very conſiſtent, or very convincing. If we are diſpoſed to grant, that the remote cauſes of fever actually diminiſh the energy of the brain, it is not an obvious inference, that the circumſtances of this diminiſhed energy have the certain effect of exciting the reaction of the ſyſtem. It appears, in ſhort, like aſcribing reſt and motion to the ſame power. But to ſmooth the prominent features of this apparent inconſiſtency, the ingenious author has thought fit to aſſume a principle, the exiſtence of which is very ambiguous in its enlarged ſenſe, and very inſufficient in its limited one. Dr. Cullen does not admit of the Italian principle of αυτοσατεια; he however aſcribes effects to the vis naturæ medicatrix, which are not capable of being explained mechanically. I mentioned before, that no efforts of nature are perceived to ariſe, under the uniform and conſtant application of a debilitating cauſe; but though this is true, I muſt likewiſe obſerve, that when theſe debilitating powers, from any cauſe whatever, actually ceaſe to act, abate materially in the intenſity of their action, or ſuffer change in its mode, before the vital principle is irrecoverably deſtroyed, nature, which perſiſts in continuing life, and even ſtruggles in attempting to maintain it, may then be ſaid to raiſe efforts, which have a tendency to reſtore the body to its ordinary health. This power, which to a certain degree, reſiſts cauſes of a deſtructive tendency and which endeavours to reſtore to their original ſtate the derangements of the ſyſtem which have actually taken place, is only a limited degree of the vis naturæ medicatrix. It is in ſhort, no more than an effort to continue the action of living; yet it is all, which we ſhall, at any time, perhaps, be able to perceive.

I have thus mentioned briefly ſome objections, to this celebrated theory of Dr. Cullen. The ingenuity of the author is acknowledged to be great; the pains and labour, which he has beſtowed in completing his favourite doctrine appear likewiſe to be conſiderable; yet I cannot help remarking, that its defects are ſtill ſo obvious, that we are unavoidably obliged to be ſatisfied with one of theſe concluſions: viz. either that the debility, which is ſuppoſed to be the cauſe of fever, is of a peculiar but unknown kind; that it ceaſes to act, or changes its mode of action from an accidental cauſe, or from ſomething in its own nature; or that a reaction ariſes in the ſyſtem, from a principle of conſciouſneſs of the deſtructive tendency of this debilitating power. It is not conſonant with the common laws of the animal economy, that reaction ſhould ariſe in that part of the ſyſtem, where the debilitating influence has been primarily and principally exerted. Suppoſitions of this nature, are only ſubterfuges, and no more in reality than myſterious ways of acknowledging ignorance.

The opinions I have enumerated above are the principal ones, which have been advanced by medical writers, on the ſubject of the proximate cauſe of fevers. Though numerous, they are all reducible to two general claſſes; viz. either to conjectures, which are totally without foundation; or to circumſtances, which are in fact only ſymptoms or parts of the diſeaſe, ſome of which are more, others leſs eſſential. The proximate cauſe of fever, is a certain peculiar ſtate of the body, on which the diſeaſe, or the ſubſequent parts of the diſeaſe, neceſſarily depend. It is, in ſhort, the firſt eſſential action of the febrile cauſe; but this action is ſo intricate and difficult to be diſcovered, that phyſicians have ſought for it in vain for more than two thouſand years. The ancients were ſatisfied with the idea of preternatural heat, excited in the heart, and communicated, by

means of the blood and ſpirits, to the reſt of the body. Hoffman, making a bolder ſtep, introduces a ſpaſmodic affection of the moving fibres; and Dr. Cullen, going ſtill farther, lays the principal ſtreſs upon languor and debility, or weakened action of the nervous energy. Increaſed heat, ſpaſmodic ſtricture and marks of debility are generally preſent, in various degrees, in the different ſtages of fever; but debility for the moſt part precedes the others; and on this account, if equally eſſential, has a preferable right to be conſidered as the firſt part of the diſeaſe. There is ſtill reaſon to doubt if it actually is the firſt. I have myſelf attended carefully to the manner in which intermitting fevers approach. The firſt thing which I obſerved in others, or what is ſtill more to be depended upon, the firſt thing I felt in myſelf, was uſually a diſagreeable, but a peculiar affection of the ſtomach. The preciſe nature of this affection I am unable to define in words; but I knew it ſo well by experience, that I always conſidered it as a warning, (and it was ſometimes the only warning, which I had,) of the approach of the paroxyſm. It was often accompanied by flatulence, and it ſometimes preceded the firſt feelings of the languor and debility, nearly the ſpace of an hour. The obſervation of this fact has occured to me frequently; and I cannot avoid concluding, that it gives room to believe, that the debility, which is ſo commonly the fore-runner of fever, inſtead of being the firſt and principal mode of action of the febrile cauſe, is only a part of that action, —perhaps not the moſt eſſential. As Hippocrates appears at a very early period to have been perfectly ſenſible, that ſomething beſides a ſimple increaſe of heat was neceſſary to conſtitute a fever; ſo we may now perhaps conclude, with equal reaſon, that debility has ſome other circumſtances combined with it, which we have not been able to diſcern very clearly.

Having ſaid that there are not any of the numerous theories, which have as yet been offered to the public on this important ſubject, in any degree ſatisfactory, it might be expected, perhaps, that I had ſomething of my own to bring forward, which might be more perfect, at leaſt in my own opinion: but I willingly acknowledge, that I have no ſuch pretenſions. After fourteen or fifteen years of careful obſervation, and daily reflexion on the phenomena of fevers, I am obliged to confeſs, that my opinion ſtill remains to be formed. The proximate cauſe of this diſeaſe, is a ſubject of a dark nature. It is ſuch, perhaps, as our limited capacities will never develope. But though we deſpair of ever attaining clear ideas of its ſpecific nature, there are ſtill ſome uſeful circumſtances connected with it, which we comprehend with clearneſs. We know, that the more general remote cauſes of fevers, are certain inviſible exhalations, ſometimes more evidently ariſing from marſhy grounds, ſometimes more obſcurely diffuſed in the air, and ſometimes obviouſly proceeding from the bodies of our fellow creatures. We know, likewiſe, that theſe cauſes which are unfriendly to the human conſtitution, are variouſly modified and combined, and of various degrees of force or in various ſtates of concentration; but we proceed no farther with certainty. We are not able to aſcertain the nature of theſe effluvia, and it is only by conjecture that we trace them in the channels by which they enter the body. The changes which they operate on the ſolids, fluids, or nervous ſyſtem, before their action becomes obvious, are totally unknown to us. We know, though the body lie expoſed to exhalation, even in a concentrated ſtate, that an appearance of diſeaſe is not, generally, the inſtantaneous conſequence. A ſpace of time intervenes, various indeed according to circumſtances, but always ſuch as gives room to believe, that the cauſe requires, and actually under-

goes a modification, before it is capable of producing a fever, or the paroxyſm of a fever. The circumſtances connected with the approach of fevers, particularly intermitting fevers, afford an illuſtration of my meaning. The cauſe of the diſeaſe, ſo far from producing the fever immediately when applied to the body, often lurks for a conſiderable time in the conſtitution, without perceptibly injuring the ordinary actions of life. Sometimes it gives riſe to affections, which are apparently very different from their real nature. Thus a perſon often languiſhes for days, weeks, or even longer. The indiſpoſition ſuddenly and unexpectedly vaniſhes: and the apparent recovery of health is ſoon followed by a paroxyſm of regular fever. In other caſes again, the attack of the diſeaſe is ſudden; and its formation from the beginning diſtinct. This fact affords a preſumption, that, in conſequence of a particular modification, which is only accompliſhed in a certain ſpace of time, but the nature of which we do not in the leaſt comprehend, an aptitude is regenerated between the remote cauſe of the diſeaſe, and the relative ſtate of the body. When the ſtate of the body, and the remote cauſe approach to, or arrive at a ſtate of mutual correſpondence, the diſeaſe is produced. When this ſtate, which I call an aptitude, is changed or deſtroyed, the diſeaſe vaniſhes, or ſuffers a change of form. This is a fact, which cannot be diſputed; and it ſeems to be the extent of our knowledge, on the important ſubject of the proximate cauſe of fevers.

CHAP. VI.

OF THE GENERAL HISTORY OF THE FEVER OF JAMAICA.

IT will not be improper to remark, before I begin to describe the history of this fever, that though the endemic which prevailed at Savanna la Mar, notwithstanding much variety of forms and symptoms, is considered as only one and the same disease, yet it may also be observed, that the variety of these appearances is sometimes so great and remarkable as to occasion considerable perplexity to the practitioner. The symptoms and form of this endemic appeared, on a superficial view to be constantly varying; yet by attending more closely to the course, progress, and changes of the disease, these apparent irregularities vanished gradually, the varieties being in fact only accidental, and often depending on very trivial causes. The analysis of the different cases of fever, which came under my care, during the time that I lived in Jamaica, furnished me with this information. I formerly mentioned the manner in which my observations were conducted: I have only now to add, that I trust the method, which I adopted, has enabled me to give a more accurate history of the fever of Jamaica, and to explain more satisfactorily than has been done hitherto the various sources of the many irregularities which are observed to occur. I am perfectly sensible that my experience has been too limited, to give me hopes of rendering the present work complete; yet I would flatter myself, that it will not be altogether useless: I totally disclaim theoretical opinions, and content myself with a plain narrative of facts; neither do I aspire to any higher praise, than care in observing

the appearances of the diſeaſe, and truth in relating the appearances which I have ſeen.

Before proceeding to give a particular hiſtory of the different varieties of the endemic fever of Jamaica, it will not be improper to mention the more general ſymptoms, which diſtinguiſh the diſeaſe, and to trace an outline of the courſe, which it has been commonly obſerved to purſue: and I may remark in the firſt place, that though debility is uſually conſidered as the firſt ſign of an approaching fever; yet, if we attend minutely to all the circumſtances of invaſion, it will not generally be difficult to perceive, that a diſagreeable, though undeſcribable affection of the ſtomach, takes place previous to the ſmalleſt perceptions of languor or debility, which are commonly only immediate fore-runners of coldneſs and ſhivering. This coldneſs, which was obſerved to be various in duration, as well as in degree of force in the fevers of this country, was ſucceeded by fluſhings of heat alternating with the cold, and increaſing gradually till the heat at laſt prevailed. The hot fit, which was likewiſe of various duration and of various force, had many new ſymptoms joined with it, the principal of which were ſuch as ſhewed an increaſed circulation, or an irregular determination of the blood to the different parts of the body. This hot fit, and the diſturbances connected with it, according to circumſtances, continued for a longer or ſhorter ſpace of time; at laſt ſweat breaking out on the head and breaſt, extended itſelf gradually to the extremities, and accompliſhed after a certain continuance, either a total remiſſion of the fever, or a conſiderable abatement of the violence of the ſymptoms. It is almoſt unneceſſary to mention, that this remiſſion or abatement of ſymptoms was of longer or ſhorter duration, and more or leſs complete in fevers of different forms. An aggravation of ſymptoms ſucceeded to the re-

miſſion; but it was uſually obſerved to begin without preceding coldneſs, and frequently without marks of preceding languor or debility. The hot fit now ran high, and all the ſymptoms were frequently more violent than they had been obſerved to be in the firſt paroxyſm. Sweat at laſt made its appearance, followed in moſt caſes by a remiſſion, leſs perfect indeed, than the preceding one, but ſtill diſtinct enough to be clearly traced. In this manner things went on for a longer or ſhorter ſpace of time, the paroxyſms uſually increaſing in violence, and the remiſſions becoming ſometimes more, though in general leſs perfect, as the diſeaſe advanced in its progreſs. I may further obſerve, that there was occaſionally a change of the type, ſometimes a change of the nature of the ſymptoms in the courſe of the illneſs; and that, where either of theſe were the caſe, the diſeaſe was uſually of longer continuance; at the ſame time, that the order of the critical days was diſturbed in conſequence of theſe changes.

The reſemblances, which I have mentioned above, were found in all the different ſpecies of the remitting fever of Jamaica; but from cauſes, which were not always perceived, and which ſometimes appeared to be very accidental, the diſeaſe was diſtinguiſhed in a part, or in the whole of its courſe, by the prevalence of a train of ſymptoms of ſuch a particular nature, as gave occaſion to the diſtinctions of inflammatory, nervous, malignant, putrid or bilious; the ſeparate hiſtories of which I ſhall now relate more circumſtantially.

SECTION I.

OF FEVER DISTINGUISHED BY SYMPTOMS OF INFLAMMATORY DIATHESIS.

THE variety of fever, which I shall describe first, is that, where the inflammatory diathesis prevailed in different degrees. Where this diathesis was moderate, the disease was usually of the least complicated form, as well as of the least dangerous nature, of any of the fevers of Jamaica. The paroxysms were generally regular, and complete in all their parts, and terminated, for the most part, by a copious sweat, in a perfect remission: the pulse was full, strong, and regular; without uncommon hardness or tension; whilst the heat of the skin, though sometimes great in degree, was generally free from that burning pungency, so common in some other species of fever. It was less removed, in short, from a simple increase of the natural warmth. I may further remark, where this moderate degree of inflammatory diathesis characterized the genius of the disease, that the danger was seldom great; and that the termination or crisis was generally regular and final. But though this degree of the inflammatory diathesis was frequently observed to be a sign of safety, and of regular crisis: yet it also often happened, where the diathesis prevailed in excess, that the symptoms of excitement ran unusually high, and that a serious danger threatened life. The pulse, in such cases, was not only frequent during the paroxysm, but it was likewise, quick, hard, and vibrating; the heat was often intense; the internal functions and the various secretions, were considerably disordered; at the same time, that a very obstinate spasmodic stricture prevailed on the surface of the body. The remission which followed, for the most part, was obscure; the

pulſe frequently retaining a preternatural quickneſs and hardneſs; whilſt there was generally a conſiderable degree of febrile heat on the ſkin.

The ſymptoms, which I have juſt now mentioned indicate different degrees of the real inflammatory diatheſis; but beſides actual ſymptoms of real inflammatory diatheſis, there were likewiſe found fevers, with the appearances of a ſimilar diſpoſition, though the real genius of the diſeaſe was in reality of a different nature. It is of importance in practice to diſtinguiſh thoſe ambiguous appearances; but it is not always eaſy to do it with certainty. We may remark, however, that the apparent inflammatory diatheſis was uſually accompanied with marks of great irritability, and ſometimes with marks of violent excitement during the paroxyſms; while languor and great depreſſion of ſpirit were frequently perceived to attend the remiſſions. The pulſe, which at one time was hard, irregular, and quick, at another was frequent and low, and ſunk under a ſmall degree of preſſure. The heat of the body was not always great, yet it was pungent,—and left a diſagreeable ſenſation on the hand: the ſecretions were often irregular; the countenance was confuſed, clouded and overcaſt, the eye was ſad, and ſometimes appeared as if it were inflamed; the feelings were unpleaſant to the patient himſelf: there was great irritability of temper; and the ſtate of the ſkin impreſſed us with the idea, that there was a ſtrong ſpaſmodic ſtricture prevailing on the ſurface of the body.—The above are the principal circumſtances, which were uſually preſent in the different ſtates of inflammatory fevers; yet theſe circumſtances were ſometimes ſo variouſly complicated and combined with others, that the accurate diſcrimination of them muſt be left, in moſt caſes, to the obſervation of the individual himſelf.

Thoſe different ſtates and degrees of the inflammatory diatheſis, which I have deſcribed above, were

ſometimes general throughout the whole of the body, not affecting one member more remarkably than another; ſometimes they were partial or ſeemed to be connected with a principal affection of a particular part: and where this was the caſe, the local affection, and the general diatheſis of the ſyſtem, uſually had a mutual correſpondence with each other. Thus, where the inflammation affected the ſubſtance of the liver or lungs, the general inflammatory diatheſis was uſually in a moderate degree; while the higheſt exceſs of general vaſcular excitement often accompanied inflammations of the membranes of thoſe organs. But though inflammation of membranes was often accompanied with a high degree of general inflammatory diatheſis; yet there were likewiſe ſome kinds of thoſe local inflammations which communicated only a low, or an ambiguous degree of their diatheſis to the general ſyſtem: ſuch are ſome of thoſe inflammations, which occaſionally affect the ſurface of the alimentary canal, and which appear, in general, to be of the eryſipelatic kind.

SECTION II.

OF FEVER WITH SYMPTOMS OF NERVOUS AFFECTION.

THE endemic fever of Jamaica, was not oftener diſtinguiſhed by ſymptoms of general inflammatory diatheſis, than by circumſtances of nervous affection. The beginning of this form of the diſeaſe, was often characterized by a high degree of that diſagreeable affection of the ſtomach, as alſo by much of that languor and debility, which are commonly fore-runners of fevers in general. To theſe ſymptoms ſucceeded a ſlight degree of chilline ſs, followed

by a hot fit, which often continued long, but seldom ran high. The pulse was small, frequent, and easily compressed. It varied with change of posture;—and sometimes was so much affected when the patient was raised upright, as totally to disappear; the heat of the body was seldom great; the secretions and exertions were generally irregular, and the internal functions were much disordered. The mind was usually affected, affected however in various degrees, and in various ways. Sometimes there was a lively delirium, sometimes the delirium was low and desponding; and, as the one or other of these was the case, the appearance of the eye and countenance was chearful or sad. The tongue was sometimes moist, sometimes dry, but seldom very foul; thirst was irregular, nausea was frequent, and the state of the stomach was generally very irritable. There was likewise, in most cases, deep and heavy sighing, and, unless in times of preternatural excitement, a very uncommon degree of despondency. The above were the principal symptoms of the nervous fever of Jamaica. The paroxysms in this disease seldom exceeded twelve hours in duration; while the termination or abatement, was usually distinguished by sweating, though seldom by such sweatings as extended completely to every part of the body. The remissions were not by any means perfect: the head-ach, and other disagreeable feelings usually abated; but signs of languor still continued, and marks of spasmodic stricture for the most part remained on the surface of the skin. I may further observe, that as the paroxysms generally increased in violence, in the progress of the fever; so it was very seldom that the remissions put on an appearance of greater distinctness, as the disease approached to its termination.

Such is the general history and the progress of the disease, which might be distinguished by the name of the nervous fever of Jamaica: but besides those cir-

cumſtances, which I have mentioned above, others were ſometimes found attending it, which, though leſs regular and conſtant, deſerve ſtill to be taken notice of. Thus the firſt ſtage of the paroxyſm, inſtead of the more uſual appearances, was occaſionally diſtinguiſhed by fits, which appeared to be of the epileptic kind. Theſe fits in ſome caſes were ſucceeded by a lively delirium, in others by ſtupor or inſenſibility. The delirium, which was a common ſymptom of this diſeaſe, ran high in ſeveral inſtances; though it more generally amounted only to an abſence of thought, or difficulty of recollection. It is a circumſtance of ſome curioſity likewiſe, that inſtead of a paroxyſm, conſiſting of different parts in a certain order of ſucceſſion, there was ſometimes a total ſtupor and inſenſibility, which continued for a determinate ſpace of time, without even being ſucceeded by obvious marks of fever: whilſt the time of the paroxyſm, in other caſes, was diſtinguiſhed by ſuch a degree of tremor and mobility, as nearly approached to the diſeaſe known by the name of St. Vitus's dance. And further, beſides theſe ſtrange and irregular appearances, ſpaſms and excruciating pains in different parts of the body, in many inſtances, were the leading, indeed almoſt the only ſymptoms of the diſeaſe.

It is not only curious, but it is indiſpenſably neceſſary in the conduct of our practice, to obſerve with attention the various modes of action of the cauſe of fevers, and to eſtimate with preciſion the various combinations. The cauſe of fevers, in exerting its principal action on the nervous ſyſtem, ſometimes produces excitement, ſometimes occaſions depreſſion; effects oppoſite to each other in their nature. Excitement and depreſſion are two general and oppoſite modes of action; yet beſides theſe we often obſerve others, which do not belong wholly to the one or the other, but which ſeem to be compounded of both, in a manner we do not very well comprehend. This

cauſe of fever likewiſe, which acts in directly oppoſite ways, appears alſo to exert its action more powerfully at different times on one part of the ſyſtem than on another; that is, it acts ſometimes more immediately on the brain, or reaſoning faculty, ſometimes more directly on the nerves, or moving powers of the body. It may even be obſerved further, that all theſe modes of action, which are preſerved diſtinct at one time, are combined in various degrees at another. Thus, where the cauſe of fever acts by producing excitement, lively delirium in various degrees is the conſequence; while languor, ſtupor, and inſenſibility naturally follow the oppoſite mode of action. Low delirium, tremors, ſtartings, &c. are probably owing to a compound effect. Both modes of action ſucceed each other rapidly; or perhaps both modes are actually preſent at the ſame time, though probably in different degrees, in the different portions of the brain. This fact at leaſt is certain, that obvious depreſſion is often combined with ſigns of great irritability. It is a remark likewiſe of conſiderable importance, that the natural functions are leſs diſordered, where the cauſe of the diſeaſe acts upon the nervous ſyſtem internally, or principally diſturbs the intellectual powers, than where this action is obviouſly external: the pulſe is then more regular, though often obſcure; the diſpoſition to faint is not ſo great; muſcular mobility is leſs remarkable, and local pains are felt leſs acutely. On the contrary, where this cauſe acts externally, or chiefly affects the moving powers, the diſpoſition to faint in changing poſture is more remarkable; tremors, ſtartings, &c. are more common; appearances, in ſhort, are more fluctuating and often more alarming.

It is a matter not leſs uſeful than curious to diſtinguiſh the different ſpecies of delirium in fevers, to trace the different combinations, and to mark the apparently trivial cauſes, which excite, or which ſome-

times remove those derangements of the reasoning faculty. It is a remark, which has been often made, that while one delirious person in fever appears only to be in better spirits than usual, another, or perhaps the same person in another paroxysm of the same disease, is outrageous or perfectly furious. A third is low and languid, absent and inattentive, or, with a fixed look of vacancy, does not seem to be otherwise deranged, than by requiring greater time to recollect himself. To which we may add, that there are some, who talk coolly on things in general; but who cannot bear mention of some particular subjects.

SECTION III.

OF FEVER IN WHICH ARE DISCOVERED SIGNS OF MALIGNITY.

THE fever of Jamaica, as distinguished by signs of inflammatory diathesis, or by circumstances of nervous affection, prevailed principally at Savanna la Mar; yet besides the above forms of the disease, there sometimes likewise occurred others, which shewed marks of peculiar malignity. It is difficult to define precisely in words the character of the disease, which I now attempt to discribe; its discriminating marks, not consisting so much in one or two symptoms, as in a certain assemblage of circumstances residing chiefly in the state of the eye and countenance of the patient, and conveyed with difficulty in verbal description. I may remark, in the first place, that there was seldom any thing very particular in the manner of invasion of this species of disease. The cold fit was rarely violent in degree, though it was often of long continuance: neither did the hot

fit uſually run high, in the common acceptation of the word, though it was ſometimes attended with circumſtances peculiarly diſagreeable. The pulſe varied remarkably. It was obſcure, or ſcarcely to be felt in ſome; in others it was ſtrong, though unequally ſo; the artery, in many inſtances, being hard and contracted, with a peculiar vibration in the ſtroke. After theſe ſymptoms and others, which are uſual in this ſtage of fever, had continued for a longer or ſhorter time, ſweat began to make its appearance on the head and breaſt, which extending itſelf gradually to every part of the body, was at laſt followed by a remiſſion, tolerably perfect for the moſt part, though there ſtill remained ſome ſtrange and unpleaſant ſenſations. It does not appear that there is any thing very uncommon in the ſymptoms, which I have hitherto taken notice of: thoſe which follow are more characteriſtic. The ſtate of the eye and countenance, afford the ſureſt ſigns of the malignity of the diſeaſe; but there is difficulty in diſcriminating thoſe appearances. The face is not unuſually flushed in fevers; but, in the preſent caſe, the countenance exhibits ſomething elſe beſides an appearance of ſimple flushing. It is likewiſe grim, dark and overcaſt, with ſuch marks of confuſion and diſtreſs, as if the patient were agitated by ſome reſentful paſſion. The eye is ſad and deſponding; and the whole appearance, in ſhort, indicates ſuch a ſtate of mind, as we ſhould be diſpoſed to ſtyle malignant. It is in ſuch a ſtate of the countenance as I have deſcribed, that the character of this ſpecies of fever chiefly reſides; yet beſides this, ſome other circumſtances frequently attend the diſeaſe, which are leſs uſual in ordinary fevers. The paroxyſm for inſtance returned, for the moſt part, much ſooner than the regular period, always with greater violence, and ſometimes with new and alarming ſymptoms. It declined in twelve or fourteen hours; but the remiſſion was leſs perfect than the preceding one; the next re-

turn of fever, which was likewiſe much earlier than the ſtated hour, was often uſhered in by convulſions, and the time of it occupied by ſtupor or coma. The tongue was likewiſe irregularly moiſt or dry. If dry, it was generally covered with a black ſcurf; if moiſt, with a thin glutinous coat, through which the red ſurface ſhining obſcurely, preſented an appearance of a leaden colour. In this caſe the mouth abounded with a ropy ſaliva. But beſides the above ſymptoms, there were alſo violent twitchings in the ſtomach and bowels, ſudden ſqueamiſhneſs, faintneſs, anxiety, reſtleſſneſs, frightful dreams, diſtreſſing apprehenſions, and frequently after the ſecond paroxyſm, a particular crouded eruption (not unlike iron-burnt bliſters,) on the upper lip, which for the moſt part ſpread towards the noſe. The type of this fever, it may be further remarked, was uſually of the ſingle tertian kind, generally anticipating by long anticipations. In moſt inſtances this malignant diſpoſition was diſcoverable at the very beginning; yet in others, no ſymptoms of a doubtful nature made their appearance till after the third revolution.

SECTION IV.

OF FEVERS IN WHICH ARE OBSERVED SYMPTOMS OF A PUTRESCENT TENDENCY.

WE meet with the term putrid fever, or fever with putreſcent tendency, in the writings of almoſt every author who has treated of the diſeaſes of hot climates: but though this expreſſion is ſo much the common language of practitioners, I cannot held obſerving, that a remitting fever, with ſymptoms of a ſpecific putrefaction, did not once occur to my

obſervation in the iſland of Jamaica, during the time that I lived in that country. I muſt however add, that though a remitting fever ſpecifically putrid is actually a rare diſeaſe; yet I do not attempt to deny, that a putreſcent tendency is frequently preſent in the primæ viæ, in a very conſiderable degree; and that marks of it are ſometimes diſcoverable, even in the general ſyſtem, at a late period of the illneſs, when the vigour of life has abated, and the powers of circulation have begun to fail. This however is ſo accidental and uneſſential, that it is only in compliance with the general language of medical people, that I think it neceſſary to deſcribe a diſeaſe, where theſe ſymptoms are obſerved to prevail. The tendency to putrefaction, which was obſerved in the fever of Jamaica, ſometimes begins in the primæ viæ; and from the primæ viæ was communicated to the reſt of the ſyſtem; ſometimes it remained confined to the limits of the inteſtinal canal, throughout the whole duration of the diſorder; in which caſe flatulence, ructus, anxiety, nauſea and thirſt were the ſymptoms which were chiefly troubleſome: the belly likewiſe was generally looſe, at the ſame time that the ſtools were dark and fetid. But where this tendency was communicated from the primæ viæ to the reſt of the body, or otherwiſe made its appearance in the general ſyſtem, a form of diſeaſe aroſe diſtinguiſhed by the following ſymptoms. If the tendency to putrefaction appeared at an early period, the heat of the ſkin made a more diſagreeable impreſſion on the hand, than was uſual in ſome other fevers; the ſkin itſelf was likewiſe for the moſt part, dry and conſtricted; the thirſt was irregular, ſometimes intenſe, ſometimes from local affection of the fauces, apparently little increaſed.—The appearance of the eye was often ſad; ſometimes it gliſtened with unuſual brilliancy; ſometimes it ſeemed to be inflamed. The countenance was generally fluſhed, often particularly confuſed, and of a grim

and clouded aſpect. I have however frequently obſerved, where ſymptoms of putreſcency diſcovered themſelves at a late period of a fever, the preceding courſe of which had been diſtinguiſhed by circumſtances of nervous affection, that the bloom of the complexion was uncommonly fine and delicate. To the above ſymptoms might be added, great irritability of temper, general uneaſineſs of ſenſation, and diſorder in all the functions of the body. When the fever aſſumed this appearance, paroxyſms and remiſſions were generally obſcure and irregular. The fever indeed often ſubſided in a ſmall degree; but the future remiſſions generally became leſs diſtinct, as the diſeaſe proceeded in its courſe. The tongue aſſumed different appearances, at different periods and in different perſons. In ſome it was moiſt, in others parched and dry. It was not univerſally foul, at leaſt it frequently happened, that the edges were clear and beautifully red in their colour. The lips likewiſe were ſometimes ſmooth, and of a cherry-like appearance; at the ſame time that the gums were inflamed and ſpongy, as they uſually are in ſcurvy: the pulſe likewiſe was ſmall for the moſt part; but it was irregularly ſo. I ſay nothing of the diſpoſition to faint in erect poſture, which though generally enumerated among the ſigns of putrid fevers by authors, does not in fact appear to conſtitute a criterion of the diſeaſe.

SECTION V.

OF FEVERS ACCOMPANIED WITH AN INCREASED SECRETION OF BILE.

THOSE ſpecies of fevers, which I have mentioned above, ſeem to affect the general ſyſtem, or every part of the body nearly alike; but beſides theſe, we ſometimes meet with others, which are diſtinguiſhed by local affections, or increaſed determinations to particular parts in a degree ſo remarkable, as to perſonate very exactly a peripneumony, a hepatitis, or inflammation of the bowels; the accompanying fever being at the ſame time ſo ſlight, as ſcarcely to be conſidered as a primary affection. As an accident ſimilar to theſe local affections of the liver or lungs, we may reckon an increaſed ſecretion of bile. The cauſe of fever, from circumſtances which we do not always perceive, ſometimes acts with particular violence on the biliary ſyſtem, in conſequence of which the ſecretion of bile being preternaturally increaſed, a diſeaſe ariſes, which without much impropriety may be called bilious. But though this irregular action of the morbid cauſe, on the biliary ſyſtem, frequently gives riſe to bilious appearances in the fevers of Jamaica; yet theſe appearances are in fact often owing to cauſes more accidental, and more remote than even this. Nauſea and vomiting are among the common ſymptoms of fevers in every country; but they are particularly frequent in thoſe of the Weſt-Indies. It is well known that a continuance of nauſea, or that a repetition of the action of vomiting, increaſes the determination, not only to the ſtomach, but likewiſe to the parts which are near to it. Hence the ſecretion of bile is preternaturally increaſed ſecondarily by the ordinary effect of vomiting, and bilious appearances become a

neceſſary conſequence of this accidental ſymptom of the diſeaſe. In thoſe two manners, viz. in conſequence of the irregular action of the morbid cauſe on the immediately biliary ſyſtem, or from a ſecondary effect in conſequence of its action on the ſtomach, the the bilious fever may, in ſome reſpects, be conſidered as a diſeaſe of nature; but beſides this, it often originates from our own treatment, viz. from the repeated uſe of emetics, or of cathartics, which are violent in their operation. The accidental appearance of bilious vomitings, in the fevers of hot climates, furniſhed medical authors with a pretence of forming a new theory, and of directing the mode of practice to a particular view. Influenced by this appearance, they aſſume it as a fact, that a vitiated quality, or a redundant quantity of bile conſtitutes the eſſential cauſe of the diſeaſe; and on this foundation adopt the plan of repeated evacuating, both upwards and downwards; a practice which evidently increaſes the ſecretion of the bile. Hence, a diſeaſe, or the ſymptom of a diſeaſe, ariſes wholly from this mode of treatment; and the removal or cure of it is afterwards attempted by a perſeverance in the means, which originally gave riſe to it:—of this I have ſeen numerous examples.

I have now deſcribed the remitting fever of Jamaica, as characteriſed by ſymptoms of a different appearance. I may further remark, that where theſe ſymptoms were unmixed with each other, there was little difficulty in the diſtinction, and little embarraſſment in planning or executing the indications of cure: but it ſometimes alſo happened, that the different ſpecies, which I have deſcribed ſeparately, was ſo perplexed and complicated, that it appeared uncertain to which kind the diſeaſe properly belonged; or to which view the practice ought to be principally directed. Symptoms of putreſcency, for inſtance, were often combined with ſymptoms of apparent inflam-

matory diatheſis; as fevers with nervous affection, or putreſcent tendency, were ſometimes accompanied with marks of peculiar malignity. It happened often likewiſe, that the nature of the diſeaſe ſuffered a total change after a certain duration; or that a fever with one train of ſymptoms ceaſed, whilſt another with a different train began.

It would be a matter of no ſmall importance, were we able to aſcertain the various cauſes, which influence the various appearances of the ſame diſeaſe; but this knowledge is not eaſily attained:—much of it indeed lies beyond the reach of our comprehenſion. We may however remark, that the ſeaſon of the year uſually has ſome effect on the diatheſis of the ſyſtem, and often on the type and form of the fever. Thus, in the dry ſeaſon, though the remiſſions are not always more perfect, the type is commonly more ſimple, and the general diatheſis is oftener inflammatory. In the rainy months, on the contrary, remiſſions are more perceivable, but the type is more complicated, and the general diatheſis of the ſyſtem has a ſtronger tendency to putreſcency, often with a mixture of ſymptoms of nervous affection, ſometimes with ſymptoms of a malignant nature. The ſtomach, bowels, and biliary ſyſtem likewiſe ſuffer more in this ſeaſon than in the drier months of the year. But beſides this difference, which ariſes from ſeaſon, we alſo find very conſtant effects from local ſituation. Thus in hilly countries there is generally more of the inflammatory diatheſis, with more frequent determination to the head and lungs, and leſs obvious remiſſions, than in flat and champaign countries, where the ſtomach and biliary ſyſtem ſuffer in a more peculiar manner.

CHAP. VII.

OF PROGNOSTIC IN THE FEVERS OF JAMAICA.

TO be able to perceive at a diſtance, the approach of danger or returning health, is a knowledge highly ſatisfactory and uſeful to the phyſician; but it is a knowledge which is not eaſily attained: for to judge with certainty of the event of fevers, requires not only long and attentive obſervation, but a diſcrimination of complicated and ambiguous appearances, which does not depend always upon attention alone. The ſagacious Hippocrates is generally conſidered as the firſt, who laid the foundation of the ſcience of prognoſtic; and we certainly muſt allow, that he has left us many important and valuable obſervations on the ſubject; yet we may alſo add, that his deciſions in many inſtances, are precipitate. Hippocrates ſeems generally to have placed too great confidence in ſigns ſeparately conſidered, and to have formed his concluſions too often on the authority of ſingle facts. Thus he has ſometimes conſidered as fatal in themſelves thoſe ſigns, which in reality are only dangerous. The abſolutely fatal ſigns in fevers are actually few in number. I am able to affirm, from my own experience, that people are ſometimes reſtored to health after many of the uſually reputed fore-runners of death are preſent. We have, in fact, as yet only an imperfect knowledge of prognoſtic in fevers; but the field is ſtill open, and careful obſervation, it is to be hoped, may enable us in time to ſupply the defects. I dare not venture to aſſert, that I have advanced beyond others in this neceſſary and difficult ſcience; but I am diſpoſed to flatter myſelf, that the following attempt to appreciate the marks of danger or ſafety in the fevers of Jamaica, may be

found in some degree useful. It contains the result of my own observations in that country; and though I am perfectly conscious that the rules are often defective, yet I likewise know, that I have suggested some hints which have not been commonly observed, and which may help to direct those, who have not had much experience of their own.

Prognostic is such, as applies to fevers in general, or more particularly to the different species of the disease. The type or form, the general course and tenor of the disorders, and the *general* nature of the paroxysms often afford useful information. From the type alone, we do not often obtain much that is to be depended upon. Long and distinct intermissions are commonly accounted signs of safety; yet we frequently see instances of the single tertian proving fatal, while types of greater complication are often void of danger: in general, however, complicated types are suspicious—and perhaps more commonly fatal than others. But though a knowledge of the type of the fever abstractedly considered, does not commonly afford any material indication of danger or safety, yet the time of the return of the paroxysm is a subject, from which more may be learned. An anticipation of an hour or two, is seldom much to be regarded; yet an anticipation of ten or twelve is always suspicious. It either indicates a latent malignity, or a tendency in the disease to change to a continued form. The complication of another fever, or the doubling of type is by no means favourable; yet it is much less to be dreaded, than a long and an irregular anticipation. Anticipations have been generally considered as signs of the increasing force of the fever; so the type which postpones, is usually believed to indicate a disease, which is hastening to a favourable termination: the effect however is sometimes the contrary. I have myself seen some instances, where, in consequence perhaps of weakness and im-

paired ſenſibility, the return of the fatal paroxyſm, though it probably had commenced ſooner, was not clearly perceived till after the uſual hour of attack. But beſides thoſe indications of danger or ſafety, which may be drawn from the nature of the ſimple type, or from the hour of return of the paroxyſm, the ſtate of the paroxyſms and remiſſions deſerves likewiſe to be attended to. It was generally obſerved where the paroxyſms were regular, and aſſumed a completer form in the progreſs of the diſeaſe, that there was not generally much reaſon to dread an unfavourable event. Hopes of ſafety might likewiſe be entertained with ſtill greater confidence, where the paroxyſms, though more violent in degree, became more regular and diſtinct after the uſe of bark, wine and ſtimulants. On the contrary, it was always an indication of danger, where they became longer or loſt the diſtinctneſs and regularity of their form, as the diſeaſe advanced in its progreſs. Changes from bad to good, in the courſe of the fever, alſo indicated more ſafety as the oppoſite changes indicated more danger, than if circumſtances equally unfavourable had continued from the beginning.

In enumerating thoſe particular ſigns or ſymptoms, from which we are led to form a judgment of the event of the remitting fever of Jamaica, I ſhall conſider in the firſt place the ſtate of the pulſe. The pulſe is ſo differently affected by the ſame cauſes in different people, and individually ſubject to ſo many peculiarities, that concluſions formed ſolely upon this baſis muſt ever be fallacious. Hippocrates, who has treated very fully of the other ſigns of prognoſtic, is totally ſilent on the ſubject of the pulſe. He has mentioned the term, indeed, in ſeveral parts of his works; but it does not appear, that he had a perfect knowledge of the nature and indications of the pulſations of the arteries. The ſubject was ſomewhat better underſtood before the time of Celſus: yet this

author does not believe, that any information could be drawn from the ſtate of the pulſe alone, which was in any great degree to be depended upon. Galen, who is generally diffuſe on every ſubject, has treated very fully of the nature of the pulſe. He has indeed multiplied diſtinctions to an amazing extent, and ſuggeſted combinations of endleſs variety; yet notwithſtanding this apparent minuteneſs, there are ſtill ſeveral important obſervations with reſpect to it, which have eſcaped him altogether. It is not many years ago, that Dr. Solano, a Spaniſh phyſician who practiſed at Antequiera, opened ſome new and curious views concerning the pulſe, and its various indications.

The detail of facts with which this writer has furniſhed us, is really wonderful, and the candour with which he has related them, independent of the teſtimony of ſeveral reſpectable authorities, engages us to give him credit. I had not heard of Solano's diſcoveries at the time I lived in Jamaica, and I do not find that I had ever taken notice of obſervations ſimilar to thoſe he has mentioned. I was able indeed, for the moſt part, to foretel from the nature of the pulſe, even in the beginning of the diſeaſe, whether the fever would be of a continued or remitting form; but I did not diſcover any ſigns from it, which led me to form a judgment of the future mode of termination. I may add, that I met not with any inſtances of criſis by hæmorrhage; neither did I ever take notice of the rebounding pulſe. The intermitting pulſe occurred frequently, ſometimes as a forerunner of of death, ſometimes as an attendant of favourable criſis: but I cannot ſay, that I obſerved that it ever preſaged a future diarrhea. I ſhall however paſs over the obſervations of others without further comment for the preſent, and content myſelf with relating thoſe circumſtances of pulſe connected with danger or ſafety, as they occurred to my own obſervation in the

remitting fever of the Weſt Indies. I muſt remark in the firſt place, that independent of peculiarities of conſtitution, a weak, a feeble and eaſily compreſſed pulſe was generally a bad one: a pulſe which was indiſtinct and ſmall, or ſmall and hard, particularly at a late period of the diſeaſe, or together with delirium or clammy ſweats, indicated for the moſt part, the moſt extreme degree of danger. That ſpecies of pulſe moreover, where the ſtroke was obſcure, or felt with difficulty, was ſuſpicious at all times; but it was particularly dangerous where accompanied with a wavering, a tremulous, a conſtantly creeping or vermicular motion in the artery. I am not certain that my meaning will be clearly underſtood; yet I believe that thoſe who have once obſerved this tremulous and creeping pulſe, will not eaſily forget the danger which it indicates. It often attended a fever of a malignant kind, where the nervous influence appeared in ſome degree, to be ſuſpended.—But to proceed: it is an obſervation ſo well known as to render any mention of it almoſt ſuperfluous, that a frequent, an irregular, a fluttering and intermitting pulſe commonly indicates danger, ſometimes approaching death: yet I muſt add, that an intermitting pulſe ſometimes attended the favourable criſis of a peculiar ſpecies of fever. It was obſerved, however, in ſuch caſes, that the pulſe was not otherwiſe irregular, than by failing in its ſtroke at the end of every third or fourth pulſation, neither was it generally found to be uncommonly frequent. Some inſtances of this ſingular appearance occurred to me during the time that I remained in Jamaica: ſo that I was in ſome degree diſpoſed to rank the intermitting pulſe among the ſigns of favourable criſis, in a ſpecies of fever, the preceding courſe of which had been diſtinguiſhed by ſymptoms of a peculiar nervous affection. When I became acquainted afterwards with the obſervations of Dr. Solano, I began to doubt whether the

intermiſſion of pulſe, which I had met with in the fevers of Jamaica, might not have been a ſign of approaching diarrhea, which had not occurred to my notice, rather than a ſign of proper criſis, as I had formerly imagined. I remained in this uncertainty till lately, that ſome inſtances of this ſymptom happening at the termination of fevers in this country, have helped to confirm me in the opinion which I entertained before. I found in thoſe caſes to which I allude, that the pulſe intermitted after every third or fourth ſtroke on the day, on which I expected the criſis. The intermiſſion of the pulſe was not of ſuch a nature as indicated approaching death; I therefore looked watchfully for a diarrhea, but no diarrhea enſued. It muſt be confeſſed, indeed, that one of the patients ſeemed to be much diſtreſſed with gripes and flatus; but being deprived of the power of ſpeech we could not obtain any accurate idea of his feelings: and no evacuation actually took place, till the day following, before which time the intermiſſion had diſappeared altogether.—Beſides the above, there are ſome other ſigns of pulſe which have their particular indications; but they are ſo generally known, that it will not be neceſſary to enlarge on the ſubject. I ſhall therefore only obſerve further, that changes from better to worſe in the ſtate of the pulſe, as the diſeaſe advances in its progreſs, are bad, while the oppoſite changes are favourable: yet I muſt likewiſe add, that in thoſe caſes of favourable change, it will be neceſſary to diſtinguiſh carefully the pulſe of coma, from the pulſe of returning health.—The difference is ſometimes ſcarcely to be known, except from the attending ſymptoms.

Next to the ſtate of the pulſe, I ſhall mention thoſe appearances of the tongue, which, together with other concomitant circumſtances, frequently afford ſigns of the mildneſs or malignity of the diſeaſe. Though we do not expect that the tongue ſhould be

of a healthy aſpect, during the continuance of a fever; yet where it is dry only in a moderate degree, or where it is covered with a ſmooth and whitiſh coat, the diſeaſe for the moſt part, is void of malignity, though not always of danger. On the contrary, where it is immoderately dry, or dry and black, the indications of danger are great, and I may add, ſtill greater where a white ſlimy and glutinous ſubſtance covers its ſurface. This ſlimy ſtate of the tongue was often ſeen at an early period, and as far as my experience goes, conſtantly indicated malignity. To the above we may add, a ſodden or parboiled appearance of the tongue, which was not of leſs dangerous import than the preceding. But beſides thoſe obviouſly unhealthy aſpects of the tongue, its appearance in ſome inſtances was not different from its natural ſtate, except in a certain lividneſs of colour. This was conſtantly ſuſpicious, and if not fatal, was always extremely dangerous. The danger indicated by the tongue, when it is intenſely dry, rough, cracked, or ulcerated is generally known; but I muſt not omit to mention, that when from a dry and unhealthy ſtate, it turns moiſt ſuddenly, or aſſumes its natural appearance, whilſt the other ſigns of favourable criſis did not ſhew themſelves at the ſame time, a change of the mode of action of the febrile cauſe is indicated,—and generally a dangerous one.—I ſay nothing of paleneſs and tremor, as theſe ſymptoms only indicate certain ſtates of general or particular debility of the nervous ſyſtem.

Vomiting is another of the alarming, and ſometimes of the dangerous ſymptoms of the fevers of the Weſt-Indies. If this ſymptom continues during the remiſſion of the fever, without material abatement, there is reaſon to dread its conſequences; but if it vaniſhes or abates in a very material degree at the decline of the paroxyſm, it does not deſerve to be ſo particularly regarded. The practice however to

which it leads is often ſerious. Vomiting is ſuppoſed by moſt practitioners to indicate emetics; but the indication is fallacious, and the practice is often pernicious. During the time that I lived in Jamaica, I had frequent opportunities of ſeeing vomitings rendered continual by the repeated uſe of emetics, which before this treatment, appeared to be only accidental ſymptoms during the paroxyſm of the diſeaſe. I therefore at laſt became cautious of purſuing this view, and have reaſon to believe, that if I did not oftener do good than others, I ſeldomer increaſed the danger. But beſides the degree and frequency of the vomiting, the nature of the matters thrown up may likewiſe furniſh indications of the danger or ſafety of the fever. The various kinds of bilious vomitings have been fully explained, and the danger of each has been ſo particularly pointed out by many writers, eſpecially by Hippocrates, that I paſs over the ſubject without further notice, conſidering it unneceſſary to repeat the obſervations of others. I muſt however remark a more uncommon kind of vomiting, which ſometimes happened in the fevers of Jamaica, and which I believe has hitherto eſcaped the notice of obſervers. The vomiting to which I allude in this place, is a vomiting of a clear and ropy liquor, in which are often found ſwimming flakes of a darker coloured mucus. This appearance was chiefly obſerved, where the remiſſions were indiſtinct, and the ſweats partial and incomplete. It conſtantly afforded an indication of danger, and I ſeldom found that the uſual remedies were effectual in reſtraining it. Vomitings of black and vitiated matters are commonly known to be of the moſt dangerous import,—ſucceeded by obſcure hickupings, they are often forerunners of death. Yet though this is generally true, I muſt not at the ſame time omit to mention, that I have ſeen ſeveral inſtances of recovery where black vomiting had prevailed for ſome time; and other

caſes which give me room to conclude, that hickupings are not conſtantly fatal. I take the preſent opportunity to remark, that hickup was ſometimes only a diſtinguiſhing ſymptom of the diſeaſe, which increaſed or declined with the paroxyſm; and that in other inſtances it attended the favourable criſis of fevers, the preceding courſe of which had been characteriſed by ſymptoms of nervous affection. This ſpecies of hickup was generally alarming in degree, and equally inexplicable with the intermitting pulſe, which I mentioned above as ſometimes attending a favourable termination. It often continued the ſpace of twenty-four hours, in ſpite of all that could be done by medicine.

Next to the indications of vomiting, I ſhall enumerate ſuch as may be drawn from the preſence or abſence of thirſt. Immoderate and unquenchable thirſt has always been reckoned an unfavourable ſymptom in fevers. It is ſo undoubtedly, yet I have frequently ſeen very extraordinary degrees of it continuing for a length of time, without particular danger. Beſides the deſire for liquid in general, there is often an unconquerable longing for drinks of a particular kind;—a feeling which ought always to be attended to, and frequently complied with.—The deſire for cold water is ſometimes ravenous.—I have known it not only ſatiated with ſafety, but even with good effects. But though this immoderate thirſt is juſtly reckoned a bad ſymptom in fevers; yet an indifference for liquid, with a dry tongue, and other marks of internal heat, is ſtill worſe. It has indeed been generally conſidered as fatal; but here we ought to diſtinguiſh, whether it proceeds from local affection of the tongue and fauces, or from a general failure of the powers of life. In the one caſe it is a mortal ſign, in the other it can only be ſaid to be dangerous.

The ancients, particularly Hippocrates and his

commentator Galen, have treated so fully of the indications of evacuations downwards, that I should be able to do little more than to copy their observations. There is one species of evacuation, however, which they do not appear to have described very explicitly, and which I have often observed to be attended with great danger. This is the frequent, small and ineffective excretion, and more particularly copious stools, which resemble dirty water, especially if accompanied with tension of the hypochondria and abdomen.

Medical writers have been long accustomed to form a prognostic of the event of fevers, from pustular or scabby eruptions about the mouth: but the sign is ambiguous, and cannot be depended upon, without many limitations. I shall however relate that which has occurred to my own observation, without troubling myself about the opinions of others. And I remark in the first place, that an eruption about the corners of the mouth, and near the lips, which comes forth freely, and soon turns into a scab, particularly if it does not appear till after the third revolution of the disease, affords a general sign of safety, at least it affords a sign that the complaint has attained the height of its violence. On the contrary, an eruption which shews itself at an earlier period, which is crouded, and makes its way with difficulty, or which resembles iron-burnt blisters rather than pustules properly so called, particularly if it is on the upper lip, and spreads towards the nose, affords a general indication of danger and malignancy.—Small and imperfect eruptions likewise are frequently a sign of a tedious disease.

The state of animal heat is another of those circumstances, which may be considered as affording an indication of the nature and event of fevers. Where the heat of the body, in the remitting fever of Jamaica, was equally diffused to the extremities,

or not differing from an increaſed degree of natural warmth, the diſeaſe was uſually mild, without particular danger or malignity; but where acrid, fiery and pungent, though perhaps not much increaſed in degree, danger was apprehended with reaſon, particularly if the warmth was not extended equally to every part of the body. In the remiſſions of thoſe fevers, which were diſtinguiſhed by ſymptoms of nervous affection, or, as is more commonly believed, putreſcent tendency, the heat of the body was often ſeveral degrees below the ſtandard of health. The ſymptom was alarming, but it was not in fact of much conſequence. This diminution of the heat of the body, during the remiſſion, was not by any means a rare occurrence; but beſides this, there was ſometimes obſerved a degree of coldneſs, during the favourable criſis of nervous fevers, of a very ſingular and extraordinary kind. In ſome inſtances this coldneſs was not inferior in degree to that of a perſon dying, or actually dead; yet a diſtinction was perceived without difficulty. It was not accompanied with marks of ſtricture on the ſurface of the body, at the ſame time that the pulſe was generally ſoft, regular and full.

Next to the ſtate of animal heat it will not be improper to conſider the indications of the various kinds of ſweats. The ſigns of a favourable ſweat are commonly known. Where that excretion was fluid, warm and univerſal, particularly where accompanied with a ſoft, full and expanding pulſe, calm and eaſy reſpiration, general relief from ſymptoms of diſtreſs, with a cheerful eye and countenance, we might in general preſume on ſafety of the diſeaſe, often on its favourable termination. On the contrary, where the ſweat was cold, clammy and partial, particularly where the pulſe became or continued frequent, ſmall and tenſe, with anxiety, reſtleſſneſs and diſturbed reſpiration, a circumſcribed flushing, a greaſy hue of the countenance, or a wild and dejected appearance

of the eye, the ſituation was then alarming :—death, in ſhort, was ſeldom far off. There is, however, an obſervation with regard to this ſubject, which I muſt not omit to mention. Authors, with one conſent, have conſidered cold ſweats as certain mortal ſigns in fevers; but there appear to be exceptions to this general rule. I met with ſeveral inſtances, while I remained in Jamaica, where univerſal fluid ſweats, of an extraordinary degree of coldneſs, accompanied the criſis of the diſeaſe. I was much alarmed when this appearance firſt occurred to me, but my fears ſoon vaniſhed, as I found that the pulſe became ſlower and fuller, that the reſpiration became calm and eaſy, and particularly that the eye and countenance acquired ſuch a cheerfulneſs and ſerenity, as are uſual at the favourable termination of fevers.

To the ſigns of prognoſtic, which I have mentioned above, I ſhall add thoſe which are indicated by the general ſtate of the vital powers, or by the more particular affection of parts, which are of immediate importance to life. Among the firſt of the affections of the vital organs, we ſhall conſider ſuch indications as ariſe from a diſturbed ſtate of reſpiration. A frequent, a hurried and unequal reſpiration, (I do not ſpeak of that which depends on primary affection of the lungs), is juſtly conſidered as a ſign of a bad diſeaſe. This is more certainly the caſe, where accompanied with deep and heavy ſighing. Frequent ſighing was a common ſymptom in the fevers of Jamaica, where the powers of life were depreſſed; and though not abſolutely a mortal ſign, it conſtantly indicated danger.

Next to the ſtate of reſpiration, I ſhall mention the ſtate of the intellect, or reaſoning faculty, which often afforded ſome prognoſtic of the event of the fever. Delirium, I obſerved before, was a common ſymptom in the remitting fever of Jamaica. Where it vaniſhed or abated as the paroxyſm declined, it was ſeldom

found to be of material consequence. On the contrary, where it continued during the remission undiminished in degree, it was a symptom of the most serious nature. I mentioned in a former part of this treatise, that the cause of fever appeared to act on the brain and nervous system, in two general and opposite ways; that is, by occasioning excitement or depression. Of these two modes of action, depression was the most dangerous; unless where the excitement ran uncommonly high. But though I observed, that there are only two general modes of operation, viz. excitement and depression; yet I must also add, that the modifications are numerous, and very variously combined. Among the most dangerous and alarming species of the derangements of the intellect, we might reckon a stern sullenness, an unmanageable furiousness, picking the bed cloaths, tracing figures on the wall, and such other instances of perverted judgment. Stupor and suspension of the nervous influence, as we might term it, were likewise greatly to be dreaded: unless they shewed themselves only during the time of the paroxysm, they were generally fatal, more certainly so, if they followed convulsions.

As nearly connected with delirium, we shall now consider other disturbed states of the functions of the brain, viz. the states of rest and watching. We do not expect that sleep should be sound and undisturbed in fevers; yet we have been accustomed to think favourably of the disease, where the patient is refreshed by it. On the contrary, total want of rest, or unrefreshing slumbers, constantly indicate danger. There is, however, an astonishing diversity of constitution in this respect, that must always be taken into the account in forming an opinion. Want of sleep was observed to give rise to delirium in some persons very speedily; others supported it for a great length of time, without any appearance of delirium or spasmodic affection. An appearance of

ſleeping, without actually enjoying the comforts of ſleep is well known to be a dangerous ſymptom; yet it is not by any means a mortal one. Anxiety and reſtleſſneſs, are often referred to the ſtomach; but reſtleſſneſs and jactitation, as depending on the ſtate of the nervous ſyſtem, were likewiſe frequent, and generally dangerous ſymptoms. Tremors of the tongue and of the hands were common appearances in fevers, with marks of nervous affection; but I have likewiſe met with inſtances, where the whole body ſhook, when any motion was attempted, not otherwiſe than it does in paralyſis or chorea ſancti viti. Startings and ſubſultus tendinum were not uncommon; and they were juſtly conſidered as indications of danger; ſometimes as forerunners of convulſion. A diſpoſition to faint, even actual fainting, was frequent in the fevers of this country. It was always dangerous, though perhaps leſs ſo, than other ſymptoms which were leſs alarming, particularly if it ſuffered increaſe and diminution with the paroxyſms and remiſſions of the diſeaſe. But beſides theſe ſymptoms, which indicate diminiſhed energies, or irregular action of the nervous influence, we may likewiſe obſerve, that the ſphincter muſcles frequently loſe their power of contracting, particularly in the advanced periods of fevers. Thus perſons ſometimes can only lie upon their back, the eyes and mouth are half open, the powers of ſpeech and ſwallowing are impaired or loſt, and urine and ſtools paſs off without conſciouſneſs or againſt their will. It is unneceſſary to remark, that theſe are all ſymptoms of the moſt extreme danger. If they proceed from a general and uniform diminution of the powers of life, we may juſtly conſider them as fatal; if they are only produced by a certain mode of action of the febrile cauſe, and are remarkably increaſed during the paroxyſm, or aggravated by peculiar circumſtances of conſtitution, we ſhall find many inſtances of re-

covery. Thus, I have often ſeen people recover, who could neither ſpeak nor ſwallow; who did not appear even to diſtinguiſh objects, and who were unable to retain their urine and ſtools; or who were not conſcious when they paſſed; yet I do not pretend to have met with any of theſe fortunate events, where theſe alarming ſymptoms were the conſequence of uniform diminution, or general extinction of the vital principle.

From the ſigns which I have enumerated, ſeparately and collectively conſidered, we may in general be able to form ſome prognoſtic of the nature and event of the fevers of Jamaica. If to theſe we add thoſe indications, which may be taken from the ſtate of the eye and countenance, we may attain a ſtill more ſatisfactory knowledge. It is an obſervation which I have conſtantly found to be true, that where the eye and countenance were ſerene and cheerful, the diſeaſe was void of any latent malignity, though it might be otherwiſe of a dangerous nature. On the contrary, where the appearance of the eye was ſad, watery, inflamed, or uncommonly gliſtening; where the countenance was of a dreary hue, downcaſt, dark and clouded; and ſometimes where it was of a beautiful blooming colour, which was not natural to the patient, there was always reaſon to ſuſpect danger. But though a ſerene and cheerful eye and countenance are generally indications of ſafety, I muſt not at the ſame time omit to mention, that it ſometimes happens in beginning mortifications, or in imperfect or unfavourable criſis, that the eye and countenance aſſume, for a ſhort time, this flattering appearance of ſerenity and compoſure, though the hour of death is actually approaching faſt. The indications from the eye and countenance are of the greateſt importance, in enabling us to form a judgment of the event of fevers; but little of this knowledge is, in fact, communicable in words. It muſt be drawn, in a great meaſure, from our own obſervations.

I do not pretend that the ſigns of prognoſtic, which I have enumerated in the preceding pages, are by any means complete, if referred to fevers in general; but I at the ſame time believe, that they are leſs defective, if applied more directly to the remitting fever of Jamaica. They were collected at a time when I was not much acquainted with books: and, on that account, I am induced to offer them to the public with more confidence; particularly, as I find that the indications, to which I have principally truſted, appeared in the ſame light to ſome authors, who are univerſally conſidered as careful obſervers, and who practiſed in climates, in many reſpects, ſimilar to that of the Weſt Indies. I have diſcriminated, as far as was in my power, between the doubtful and more certain appearances of danger or ſafety; and I hope I have no where advanced any thing, which has a tendency to miſlead the uninformed. I may add, that general knowledge in prognoſtic goes no farther than a very rude outline, which individuals muſt fill up from their own experience. There are, in fact, few ſigns in fevers, which are abſolutely deciſive in themſelves; and as theſe ſigns are often variouſly combined, ſo they muſt be ſeparately and collectively eſtimated. It is only from conſidering accurately the reſult of the whole, that we can be enabled to ſpeak with confidence.

CHAP. VIII.

OF THE DIFFERENCE BETWEEN A CRISIS AND SIMPLE REMISSION.

IT is certainly a matter of some importance, to be able to distinguish between actual crisis and the simple remission of the fever of Jamaica; but it is a matter about which the practitioners of that country did not seem much to concern themselves. A difference undoubtedly exists, and the marks of it appeared to me clear and unequivocal in most instances. It was otherwise in the intermitting fever of America. In the pure intermitting fever of that country, I sometimes guessed luckily; but I cannot say positively, that I ever discovered signs on which I could depend with absolute certainty, that the fever was gone, not to return again, till the hour of return was past. In this disease, on the contrary, I should not expect to be deceived once in a hundred times. Much of this information, however, is too minute to be intelligibly explained in words, and therefore can only be acquired by actual observation.

I shall attempt to enumerate the principal of those signs, from which we may be enabled to attain some knowledge of the difference between actual crisis and temporary remission: and in the first place I remark, that the tongue was usually rough and dry, even during the most perfect remissions of the endemic fever of Jamaica. If it therefore happened, that it assumed a smooth and moist appearance at the end of a paroxysm, there generally was reason to believe that the fever was gone, not to return again. This presumption was still stronger, where its edges acquired the cherry-like colour of health; and particularly where the coat, with which it was usually covered, shewed

a diſpoſition to looſen and ſeparate. It muſt however be remarked, that though theſe appearances of the tongue afforded a common mark of the termination of fevers, they did not by any means afford a deciſive one. Inſtead of criſis, they ſometimes only indicated a change in the mode of action of the febrile cauſe; they were, in ſhort, in ſome caſes only forerunners of ſymptoms of nervous affection, or marks of a change from a continued to a remitting or intermitting form. Such are the preſumptions of actual criſis, which might be drawn from the appearances of the tongue, where the tongue happened to be remarkably changed from its natural appearance in the preceding courſe of the diſeaſe. But it alſo ſometimes happened in caſes of the ſingle tertian, where the paroxyſms were ſlight, and the remiſſions long and perfect, that the tongue was ſo little altered by the preſence of the fever, as not to afford any certain criterion between the remiſſion and actual criſis. In ſome fevers likewiſe of a malignant kind, the tongue was ſometimes ſmooth and moiſt, even red and clear on the edges, whilſt the diſeaſe was advancing rapidly. This, however, ſo far as I have ſeen was conſtantly connected with a particular ſtate of the ſtomach, viz. with naufea, or with vomitting of a viſcous liquor.

Signs of criſis taken from the pulſe alone, were not in general much to be depended upon in the fevers of Jamaica; yet, together with other circumſtances, the ſtate of the pulſe might often help us to decide in doubtful ſituations. Changes from bad to better, if no ſymptoms of comatoſe affection appeared at the ſame time, were generally conſidered as indicating criſis, or tendency to criſis; yet it will be leſs expected, perhaps, that I ſhould rank the intermitting pulſe among the ſigns, which indicate a favourable termination of the diſeaſe. Some inſtances of this have occured to me, both in the Weſt-Indies and in England; but though I mention the fact, I confeſs myſelf unable to offer an explanation of it.

The ſtate of the ſkin affords marks more deciſive of the total or temporary abſence of fever, than the ordinary ſtate of the pulſe. When the ſweat, which in the preceding remiſſions had been partial and imperfect, became copious, fluid, univerſal and of long continuance, there was generally a preſumption of criſis. But independent of the nature of the ſweat, there is ſomething in the ſtate of the ſkin, ſomething in the impreſſion which it makes on the hand which feels it, very different when the fever has only remitted, and when it has terminated finally. Though it may be difficult, perhaps impoſſible, to mark this diſtinction preciſely in words; yet it is eaſily known to thoſe, who accuſtom themſelves to obſerve minute circumſtances with attention. There was, in fact, no indication of that ſpaſmodic ſtricture on the ſurface, which had been obſervable in the former remiſſions, and we might ſay, that the body was perſpirable, even in the extreme parts. It is a circumſtance likewiſe not a little curious, that the heat of the body, during a criſis, particularly in thoſe fevers, which had principally affected the nervous ſyſtem, was ſometimes ſo much below what it uſually is in health, as to be really alarming. In ſome inſtances, I have found the extremities to be not leſs cold, than if the patient had been actually dead; yet this coldneſs was of ſuch a nature, as to be diſtinguiſhed without much difficulty from that which precedes death.

The above is only a very imperfect hiſtory of thoſe ſigns, which actually diſtinguiſh the criſis of fever from a ſimple remiſſion. There are ſtill ſome others, not leſs to be depended upon, perhaps, but which cannot be ſo eaſily reduced to diſtinct deſcription.—Among theſe, we may reckon unuſual evacuations upwards or downwards, ſound and refreſhing ſleep, where watchfulneſs had prevailed through the preceding courſe of the diſeaſe, return of natural appetites, decreaſe of thirſt, looſening of ſcabby eruptions, and

above all, a certain expreſſion of chearfulneſs in the eye and countenance, which though not to be defined in words, conveys to the mind of the obſerver, a ſtrong conviction of what is going forward. This brightneſs of the eye was well known to Hippocrates, as a ſalutary ſign in fevers; but though it generally affords a very decided indication of a favourable event; yet we muſt be careful to diſtinguiſh from it that clear and glaſſy appearance, which the eye ſometimes acquires previous to death.

CHAP. IX.

THE GENERAL CURE OF FEVER.

BEFORE I attempt to offer rules for the treatment of the remitting fever of Jamaica, it will be proper to conſider in the firſt place, how far the cure of the diſeaſe is the work of nature, and how far it already has been, or hereafter may be accompliſhed by the exertions of art. The queſtion is important, and till its limits are defined, we cannot hope to eſtabliſh rules of practice on a firm baſis, or to conduct a mode of treatment on a conſiſtent plan. It will be neceſſary however, before entering farther into this ſubject, to conſider the power of the vis medicatrix naturæ :—a principle, which under one name or other, has influenced the views of medical men from the earlieſt records of phyſic, till the preſent times.

SECTION I.

OF THE VIS MEDICATRIX NATURÆ.

IT is an opinion, which ſeems either to have been expreſsly avowed, or tacitly acknowledged in every age of the world, that a fever is a combat or effort of nature, to remove from the ſyſtem the derangements of a morbid cauſe; or in other words, to reſtore a diſeaſed body to its ordinary health. It was long believed, that the powers of the conſtitution made an attempt to concoct the crude and undigeſted humours;—and finally to expel them from the body. But this mode of reaſoning is found to be unſatisfac-

tory: and ſome late authors have adopted the idea, that nature directs her efforts towards effecting a ſolution of ſpaſm, on the preſence and obſtinacy of which the diſeaſe has been thought, in ſome meaſure, to depend. It will be a hard taſk to reconcile this difference of opinion, or to ſay which is the right one. There are plauſible arguments on both ſides of the queſtion; and neither ſuppoſition, perhaps, is accurately true. The coction of humours (as it may be called) is often apparently connected with evident approaches towards a relaxation of ſpaſmodic ſtricture; and increaſed diſcharges by the different excretories, are obviouſly attendants of its actual ſolution. Yet though this is certainly true, it ſtill is not clear, that either the coction of the humours, or the ſolution of the ſpaſm, is the real effect of a regular mechanic operation of the powers of nature, attempting by this means to overcome the deſtructive tendency of the diſeaſe. It is even more probable, that the coction of the humours, or ſolution of the ſpaſm are only circumſtances of accident, occaſionally connected with certain ſtates of action of the morbid cauſe; but which do not ariſe from the regular deſign of nature to accompliſh this purpoſe. I juſt now obſerved, that there is a difference of opinion about the mode of operation, which nature employs to combat the effects of the diſeaſe; but I may add, that no body, as far as I know, attempts to deny the exiſtence of ſuch a principle in the conſtitution of the frame, as diſpoſes it to reſtore its own health by a certain train of regular efforts. On the firſt view of the ſubject, indeed, there are many circumſtances which give countenance to the opinion. The relief which often follows hemorrhage, ſweat and other evacuations in unuſual quantity; and ſtill more, the relief, which attends the eruption of the ſmall pox, or the appearance of gout on the extremities, undoubtedly affords a ſtrong preſumptive argument, that nature raiſes

ſome active and generally well directed efforts, which remove from the body a cauſe that diſturbs the ordinary functions of health. But though the above circumſtances are commonly known, and may be ſuppoſed to afford an argument in ſupport of this opinion; yet the fact may perhaps admit of another explanation, while there are other appearances, connected with the ſubject, which render the exiſtence of this regular deſign of nature very queſtionable. When I ſay, that I cannot readily allow the vis medicatrix naturæ, (in the ſenſe in which it is generally underſtood), to be an eſtabliſhed principle in the conſtitution of the frame, I am aware, that I incur an imputation of of preſumption. An opinion, venerable from antiquity, and ſupported by many plauſible arguments, might be thought to be ſecure from the attacks of a man, who has no profeſſional reputation to boaſt of: yet as no deſire of novelty has induced me to fabricate a conjecture on a dark ſubject, ſo I humbly hope I may be indulged in my attempt to explain an important truth. The ſanction of two thouſand years, and the authority of the names who ſupport this doctrine, are formidable opponents; yet I truſt I may be able to prove, that the vis medicatrix naturæ does not, as is commonly believed, reſtore the health of the body by one general and uniform mode of operation; or that our diſeaſes are not removed in conſequence of a regular deſign in the mechaniſm of the frame. I ſhall relate the fact from which I have been led to form this concluſion; for the refutation or confirmation of which, I require no other indulgence than a candid enquiry.

I have juſt now declared, that fever, or the cauſe of fever, is not combated and finally overcome by a regular train of active efforts, or a vis medicatrix naturæ: and I muſt obſerve in proof of it, that there were many of the fevers of the Weſt-Indies, where the diſeaſe, or the paroxyſm of the diſeaſe, vaniſhed or

declined, without any exertion on the part of nature being perceiveable. The powers of life, during this period, were in some degree suspended. The patient, who could only be said not to be actually dead, was totally insensible to every object that was near to him; and often did not feel the irritation of acrid substances that were applied to him: yet after a certain continuance in this state, he began to resume his powers of sensation and motion; and the disease at last subsided or vanished, though the efforts of nature were not discovered; nay, though the vital powers were sometimes in a state so weakened, as to be apparently very little capable of effort. This fact, which the most superficial observer could not easily overlook, furnishes sufficient reason for doubting of the very existence of a vis medicatrix naturæ; a doubt which is further corroborated, by observing the manner in which death frequently approaches in the fevers of Jamaica. It is known, that the sufferings of the patient are sometimes alleviated for a short time before death. This alleviation, wherever it does take place, happens constantly at the expected period of crisis. The cause of it has generally been attributed to the vis medicatrix naturæ; that is, to a last effort of the powers of life; but I have weighed carefully all the circumstances connected with the phenomenon, and cannot readily assent to the opinion. To indulge in conjectures, is contrary to the principles I profess; yet I must suggest, that a fever, or the paroxysm of a fever, terminates, strictly speaking, from a hidden something in the nature of the febrile cause, from something which ceases to act, or which changes its mode of action after a certain duration. I do not pretend to explain the manner in which this happens. I only add, that the fact is supported by probabilities. It was frequently observed, in such cases of fever as terminated fatally, that there was actually a period of time, generally the period immediately preceding a

decided fatal termination, where the real preſence of diſeaſe was perceived with difficulty. The patient, however, was unable to recover. Death happened in a very few hours, and it ſeemed to enſue in ſuch caſes, from one of the following cauſes, viz. either from the mechaniſm of a part of vital importance being deſtroyed; from the powers of nature being too much exhauſted to continue life; or perhaps ſtill oftener, from the recurrence of the diſeaſe, in another form, ſpeedily putting a period to exiſtence, while the vital principle was in this weakened ſtate. But though the circumſtances, which I have mentioned, afford grounds for believing that a fever, or the paroxyſm of a fever, is not actually removed from the body, ſolely by the efforts of a vis medicatrix naturæ; yet if we chooſe to proceed further in the inveſtigation, it will be no difficult taſk to involve this opinion, which has hitherto been conſidered almoſt as ſacred, in ſtill greater perplexity. If we admit of the exiſtence of a vis medicatrix naturæ, it will not be eaſy to conceive, how a fever, which has once been expelled from the body, ſhould return again in a given time, or how the alternate paroxyſms of the double tertian, for inſtance, ſhould be of ſuch different duration or of ſuch different degrees of violence in the ſame perſon; neither can we underſtand, how a fever of one kind ſhould laſt only ſeven days, another fourteen, and another twenty, or longer:—circumſtances which happen daily, without the leaſt apparent connexion with the innate vigour of conſtitution. We ſhall be equally puzzled likewiſe, if we attempt to explain on the ſuppoſition of this principle, how a fever ſhould continue, while the powers of the conſtitution are vigorous and ſtrong, and ceaſe when they are apparently exhauſted. The above are well known facts, and do not leave any room to doubt, that the termination of fever, or of the paroxyſm of fever, depends on ſome other principle beſides the mere efforts of the vis medicatrix

naturæ. Whether this reſides, as was hinted before, in a hidden modification of the nature of the cauſe, which ceaſes to act, or changes its mode of action after a certain duration; or whether, combined with this, the conſtant but imperceptible changes, which are continually going on in the ſyſtem, deſtroy in the frame of the individual, that particular ſtate of aptitude to the febrile cauſe, in which the diſeaſe conſiſts, we cannot determine with certainty; yet it would be obſtinate to maintain any longer, that the cure of fever is owing to general and well directed efforts of nature, expelling a morbific matter or overcoming a prevailing ſpaſm. It is true, that an obvious ſolution of ſpaſmodic ſtricture, or the appearance of a morbid matter on the leſs important parts of the body, uſually attend the favourable termination of fevers; yet theſe circumſtances are, in fact, attendants rather than cauſes of criſis. I do not deny, that increaſed diſcharges by different outlets, ſometimes moderate the violence of fever during its continuance, as well as attend its final ſolution; yet it has not, nor perhaps can it ever be demonſtrated, that this proceeds from a regular deſign of nature.

The hints which have been thrown out in the preceding pages, give room for ſuppoſing that the vis medicatrix naturæ, in the ſenſe in which it is uſually underſtood by medical writers, is only a principle of doubtful exiſtence in the conſtitution of the frame; yet though this is certainly true, I do not pretend to deny, that the animal machine is endued with a power, which reſiſts, in ſome meaſure, the derangements of a deſtroying cauſe, and which perſiſts to a certain degree in continuing the action of living. The general nature of the cauſe of fever, or the nature of its various modifications is a myſtery, which we do not as yet know. We only know, that when preſent in a certain ſtate of vigour and activity, it deranges or diſturbs the actions and functions of the ſyſtem;—

while we likewiſe know, that it does not always diſturb every action or every function in the ſame degree. It has occurred too often to have eſcaped the moſt ſuperficial obſervation, that where one part of the body ſuffers particularly, the others are often relieved in proportion. We frequently in this manner obſerve, that general fever is diminiſhed by the appearance of local pain; or, on the contrary, increaſed by its removal. It likewiſe often happens from the ſame principle, that where the ſtomach and biliary ſyſtem ſuffer much, there is leſs diſorder in the other parts: and on the other hand, that where theſe ſufferings are removed or mitigated, the general fever runs higher, and often continues high, till the ſame, or other local affections, are again produced. Thus, though we are totally ignorant of the intimate nature of the cauſe of fever, we ſtill perceive very plainly, that it either poſſeſſes ſomething in its own nature, or accidentally meets with ſomething in the conſtitution of the individual, which determines it to affect the different parts of the body in an unequal degree. It uſually exerts its greateſt force upon parts, which are preternaturally weakened by the general influence of climate, ſeaſon, ſituation, or other accidental cauſes. Hence bilious appearances are common in the hot months of hot climates, pneumonic affection in cold and dry weather, greater degrees of vaſcular excitement among the temperate and more active races of men; while ſymptoms of nervous affection prevail among the luxurious and enfeebled. The above, with other ſpecies of the increaſed action of the cauſe of fever on a particular part of the body, depend wholly, perhaps, on circumſtances of accident; yet it has ſo happened, that thoſe irregular determinations have unfortunately been conſidered as the efforts, which nature employs to expel from the body a cauſe, which diſturbs the economy of health. I ſhall not, at preſent, go ſo far as to contend, that

theſe determinations are not, in fact, intentions of nature; but ſhall only beg leave to ſuggeſt, if they actually are intentions, that it is mere chance which determines whether they are ſalutary or fatal. It is a truth which nobody will deny, where the force of the diſeaſe is accidentally directed to an organ of excretion, or to a part of little importance to life, that the reſt of the body is often proportionally relieved, and even that a recovery of general health is ſometimes the conſequence; yet the contrary is the effect, where the functions of the part, upon which the force of the fever has been thus accidentally diverted, are of immediate importance to the action of living. The Gout, a diſeaſe, the cauſe of which bears no very remote analogy to the cauſe of fever, may be adduced as affording an illuſtration of this truth. The proximate cauſe of gout, is equally hid from us as the proximate cauſe of fever. We know, however, that the one equally with the other, has a tendency to deſtroy life. We likewiſe know, that there is a power or principle in the conſtitution, which to a certain degree reſiſts deſtruction. The nature of this power, however, is unknown. We are not only in the dark with regard to its nature; but we can only form conjectures about the part where it principally reſides.—We, however, clearly perceive its force and activity to be different in different parts of the body. We may next be allowed to remark, that where the cauſe of gout is in a certain ſtate of modification, tumults, (which properly enough may be termed re-action), ariſe in the ſyſtem, and go on to continue till this cauſe or hurtful matter finds an outlet from the body, or a lodgment on one particular part. The outlets from the body are numerous: the parts on which the gout ſeems principally to fix its ſeat, are the extremities, where the power of reſiſtance is ſmalleſt.—The vital principle, however, becomes weaker as man advances in years; and the cauſe of the diſorder

ſeems then frequently to find accommodations in parts, which are leſs remote from the ſources of life. This more eſpecially is the caſe, where tone and vigour have been preternaturally weakened. Hence the ſtomach, the bowels, ſometimes the brain, and even the heart itſelf ſuffer from the immediate action of this diſeaſe, in the latter periods of life. But though no perſon perhaps will deny, that the cauſe of gout finds readieſt accommodation, (if I may ſo apply the term) in thoſe parts of the body, where the vital powers are naturally weak, or have been accidentally weakened from various cauſes; yet we may add, that it is likewiſe removed from the parts, on which it has been thus fixed, by ſuch applications as excite their active powers; or, in other words, which call forth the local re-action of the ſyſtem. We may alſo obſerve, that tumults ariſe in the general ſyſtem, in conſequence of this repreſſion or repulſion of the morbid cauſe from a particular part; and that they do not in general ceaſe, till an outlet is opened, or accommodation found in ſome other parts of the body. The above appearances, occur daily in the hiſtory of gout. They ſeem to bear a ſtrong analogy to thoſe irregular determinations, which frequently take place in fevers, and their cauſe perhaps is the ſame. We do not perceive any other law by which they can be explained, than the natural or adventitious ſtate of activity of the powers of life, which reſiſt deſtruction with unequal force in the different parts of the ſyſtem: ſo that we ſhall be obliged to conclude, that thoſe ſufferings, which have hitherto been ſtyled the efforts of nature, are in reality more of the paſſive, than of the active kind.

The circumſtances which I have now mentioned, combat the very exiſtence of the opinion, which has been commonly received with regard to the vis medicatrix naturæ. I have hinted, that the extent and limits of that principle are narrow, and that the ſa-

lutary effects are accidental. I shall next endeavour to shew, that they cannot, without danger, be made the basis of the general plan of cure in febrile diseases. The task is important, but the attempt may be thought presumptuous, as an opinion, contrary to that which I advance, has obtained almost the universal consent of mankind. I have no desire of changing names, or of making distinctions, where there is in fact no difference. I perfectly acquiesce in retaining the word vis medicatrix naturæ, provided it is limitted to a certain mode of re-action, or to a power in the constitution of resisting destruction unequally in its different parts, in consequence of which, irregular determinations sometimes prove salutary by accident; yet I must add, that if we mean to denote by this term a system of laws, which have the best directed tendency to remove from the body a cause which destroys health, and endangers life, the opinion has a very uncertain foundation. There are few persons so ignorant, or so blindly devoted to the doctrines of αυτοκρατεια as not to own, that the usually reputed efforts of nature, are often ill directed, sometimes pernicious: in short, that they are obviously the causes of death. The truth of this observation cannot be denied, and unfortunately it obliges the advocates of the vis medicatrix naturæ, to grant the conclusion, that the laws of the principle are imperfect. The works of the author of nature, as far as our limited knowledge can trace them, are universally without defect, if examined according to the plan on which they have been originally formed. If they appear otherwise, it becomes us to hesitate, before we decide. We may not have comprehended the fundamental principle of the design; but we revolt from the idea, that the execution would be left imperfect, had it been intended by the Author of our being, that the mechanism of the frame would be such, as should oppose and remove, in the most

effectual manner, the derangements of the morbid cauſe. Defect and imperfection can have no place in the works of the Almighty. Had it actually been in the original deſign of our Creator, that the human body ſhould be provided with a ſyſtem of the beſt concerted laws for reſtoring its health, when deranged by the numerous cauſes of diſeaſes, as it is impious to ſuppoſe, that thoſe laws could be defective; ſo we may reaſonably conclude, that the effects of fevers would not then have been fatal. We find however, that fevers, as well as other diſeaſes, are fatal to people of all ages and deſcriptions: and that nature's intentions of cure, if they really are intentions, are often deſtructive to herſelf. I need ſcarcely remind the reader of examples of their pernicious tendency. Vomitting, ſweating, increaſed diſcharges by ſtool, &c. are generally conſidered as the ſalutary efforts of nature: but inſtances are numerous, where the exceſs of thoſe evacuations have obviouſly proved the cauſes of death. In the ſame manner, abſceſſes, which in the remote parts of the body, ſometimes attend, and even ſometimes perhaps influence the favourable termination of fevers; in the brain, or in other organs of importance, are no leſs certainly the cauſe which deſtroys life. In both inſtances the deſign of nature, if it can be called a deſign, is the ſame. The force of the diſeaſe being turned principally upon one part, the reſt of the body is in a great meaſure relieved from its ſufferings; but the health and ſtructure of the part are hurt or deſtroyed by the change; and it depends wholly upon the accidental importance of the organ, upon which this diverſion has been made, whether death or recovery is the conſequence. Thus it often happens, that the reputed indications of nature prove the immediate cauſes which deſtroy the exiſtence of the individual; a fact not reconcileable, with the infinite power and wiſdom of the Author of our being.

I have inſinuated, that the efforts of nature are uncertain and precarious. They depend on accidental determinations to different parts of the body; and I may add, that if we endeavour to inveſtigate the cauſe, which directs the mechaniſm of the frame, to adopt one ſpecies of effort, or one mode of determination in preference to another, we ſhall not perhaps be able to find any other, than a difference in the ſtates of the powers of life, which reſiſt deſtruction with unequal degrees of force in the different parts of the body. Where there is the leaſt reſiſtance, either from the natural or accidental circumſtances of the conſtitution, there the diſeaſe moſt obviouſly exerts its greateſt force. Hence we are ſufficiently warranted to conclude, that though the ſtructure of the human body is perfect with reſpect to every purpoſe for which it is intended, being only endued with a principle, which reſiſts deſtruction, or perſiſts in continuing life to a certain degree; yet that it is extremely defective, if we conſider it as a machine furniſhed with a ſyſtem of laws, which have an invariable and well directed tendency to reſtore health by the moſt judicious and rational efforts. The reſtoration of health, in conſequence of this re-action, or irregular determination which takes place in the ſyſtem, is only a circumſtance of accident. The ſkill of man ſometimes ſucceeds, where the efforts of nature have obviouſly failed.

SECTION II.

OF THE GENERAL INDICATIONS OF CURE IN FEVERS.

THE vis medicatrix naturæ, has been hitherto eſteemed a principle of much importance in the cure of febrile diſeaſes. I have attempted to ex-

plain its real limits and extent; but am afraid I may not have done it ſatisfactorily. A tumult which, properly enough perhaps, may be termed a reaction of the ſyſtem, evidently takes place in conſequence of the application of a morbid cauſe; but there ſeems to be little reaſon for believing, that this reaction points out the beſt method of cure, or wholly by itſelf accompliſhes this important buſineſs. But though the reputed efforts of nature are thus defective in accompliſhing the cure of fevers; yet I do not deny, that there is a general tendency in fevers, or in the paroxyſm of fevers, to terminate in a given time, often by a fixed and regular mode of termination. We do not, however, by any means comprehend the cauſe upon which this depends. From the ſimilarity in the progreſs and termination of epidemics, as well as from the ſteadineſs with which various ſorts of fevers purſue their courſe, in ſpite of the moſt oppoſite modes of treatment, we are led to conclude, that there is ſomething peculiar in the modification of the cauſe, which influences the duration of the diſeaſe. This at preſent, is unknown; perhaps is a knowledge which we cannot attain; yet if we take pains to obſerve the courſe of fevers with attention, we may diſcover ſome rules of practical uſe. We know that one ſpecies of febrile diſeaſes, obſtinately purſues its courſe, notwithſtanding every endeavour to oppoſe it; while another is ſo totally under our management, as to be ſtopt ſhort at pleaſure with almoſt infallible certainty. It thus happens, that the intermittent is perfectly under our controul. Over the continued, and even over the remitting fever of Jamaica, I am afraid, we ſhall be obliged to confeſs, that we poſſeſs no very certain power.—But I ſhall examine this ſubject more particularly.

When I firſt arrived in Jamaica, in the year 1774, I found that the practitioners of that country very generally believed, that the courſe of the ordi-

nary endemic fever was checked with great certainty by the powers of Peruvian bark. This opinion, indeed, is found in every medical book, and it appeared frequently, on the firſt view of the ſubject, to be well founded. No great ſpace of time, however, elapſed before ſome circumſtances were obſerved, which preſented the matter in a different light. I found in many inſtances, that bark was given in the firſt remiſſion, or on the ſecond day of the diſeaſe; in others, it was not given till the third remiſſion, or till the ſixth day from the beginning of the complaint; and in ſome again, the fever diſappeared altogether before a ſingle grain of this remedy had been adminiſtered. I was particularly exact in marking the time or the period of the diſeaſe, at which the bark was begun to be given, as alſo the quantity which was taken upon the whole. The reſult was not ſuch as might have been expected. Notwithſtanding the moſt oppoſite modes of treatment, the diſeaſe appeared to terminate or change about the ſame periods in almoſt every patient. This fact was confirmed in numerous inſtances; and it ſeems to afford a very unequivocal proof, that bark, in the quantity in which it is commonly preſcribed in the Weſt Indies, has not the effects which are *uſually* aſcribed to it. I do not, however, infer, that this remedy may not be capable, with more deciſive modes of management, of effecting all that has been expected from it. I had not, during the time that I remained in Jamaica, any conception that the ſtomach could have retained, or that it would have been ſafe to have ventured upon the quantities of bark, which I afterwards gave to others, or took myſelf in the intermitting fever of America. Two ſcruples or a drachm, every two hours, is in fact only a ſmall doſe. To this under doſe, during ſhort remiſſions, we might perhaps impute the failure of that remedy in the fevers of the Weſt Indies. That this actually is the caſe, is confirmed

in some degree, by an instance which I find recorded among my notes. A young man was seized with a fever, about the middle of August, which shewed marks of great violence from the beginning. Bark was given early, and in larger quantity than customary. The last paroxysm of the disease, was in some measure suspended, in consequence of this proceeding; yet, except that the marks of external fever were obscure, the patient remained, as usual, uneasy and distressed, till the period at which the crisis was expected; when the marks of final termination shewed themselves distinctly. This is the only case I met with, where the paroxysms of the fever of Jamaica were stopt, or suspended by the bark; or where external marks of fever vanished without evident signs of crisis. It affords only a doubtful proof of the power, which this remedy has been supposed to possess, of absolutely cutting short the course of the endemic of that country. But though the bark was seldom efficacious in abruptly cutting short the course of this disease, it is no more than justice to remark, that it is a remedy which was almost every where safe, and that it was ultimately useful in promoting the cure. It imparted in most instances, where it was employed, a degree of tone and vigour to the system—a certain something to the constitution; in consequence of which, the crisis, which we should have expected to be only partial or imperfect, became decided and final. I have suggested those few remarks, with regard to the virtues of Peruvian bark in the common endemic of Jamaica; if its effects are so very doubtful in this disease, we have no reason to expect, that they will be more certain in fevers of a more continued kind.

Besides bark, the power of which appears to be very precarious, other remedies have been employed by physicians, with the view of cutting short the course of fevers.—Antimony, under one form or

other, has been celebrated for this intention, ſince its firſt introduction into medicine. James's Powder is the moſt famous, and perhaps the moſt effectual antimonial preparation, which, as yet, has been offered to the public. I am ſorry to ſay, that I had not an opportunity of making proper trial of it, in the fevers of the Weſt-Indies; but I can add, that the emetic tartar was often found to be dangerous, ſcarcely ever effectual in cutting ſhort the courſe of the diſeaſe, unleſs given at an early period, or before the fever had aſſumed a proper form. Its *virtues*, as a febrifuge, were heightened by the addition of opium and camphire, I am ſorry alſo to remark, that I cannot ſpeak with confidence of the virtues of James's Powder, in the intermitting fever of America. Emetic tartar was frequently employed, but it did not by any means anſwer the expectations which were entertained of it. I acknowledge, that it might be ſo managed, as apparently to prevent the return of a particular paroxyſm; but the inſtances, where it completely removed the diſeaſe, were ſo rare, that I do not conſider it as poſſeſſed of very eminent virtues. I have had frequent opportunities, ſince my return to Britain, of trying James's Powder in the continued fevers of this country; and the reſult of my experience inclines me to believe, that this remedy, when given at an early period, has ſometimes actually carried off the diſeaſe. It appeared likewiſe, when exhibited near the critical periods, to render the criſis more complete; but I have little cauſe for thinking, that it ever cut ſhort a fever in the midſt of its courſe. Thus it appears, that theſe two celebrated remedies,—bark and the various preparations of antimony, are, in fact, leſs effectual in cutting ſhort the courſe of febrile diſeaſes, than has been commonly ſuppoſed; I cannot, however, abandon the idea that the purpoſe, which has been expected from theſe remedies, may ſtill be obtained by other means.

Galen mentions ſome inſtances, where he extinguiſhed the fever by copious evacuations: authors mention many, and I have myſelf ſeen ſome, where drinking plentifully of the coldeſt water, produced the ſame effects. The alternate uſe of warm and cold bathing occaſions great changes in the ſtate of the conſtitution: and from the trials, which I have made of theſe applications, I do not entertain a doubt, that they may be ſo managed, as to ſhorten very materially the duration of fevers. I do not however promiſe, that they are capable of being ſo conducted, as infallibly at once to ſtop the diſeaſe in its progreſs. This can only be accompliſhed by thoſe great and remarkable changes, which deſtroy a certain aptitude, in the ſtate of the ſyſtem, to the morbid cauſe, in which the diſeaſe is ſuppoſed to conſiſt. But I muſt at the ſame time confeſs, that as we neither know the nature of this aptitude, nor the particular nature of remote cauſes, ſo every attempt of cure on this plan, as it muſt be at random, cannot be adopted without danger. It is a view, therefore, which will not be proſecuted with ſafety, while our knowledge of the nature of morbid cauſes, and of the laws and ſtructure of the human frame, is ſo imperfect.

It is evident from the facts which I have related in the preceding pages, that we cannot ſafely truſt the cure of fevers to thoſe tumults, or irregular determinations in the ſyſtem, which are uſually ſtyled the efforts of nature: neither does it appear, that we can depend on the efficacy of any one remedy, we are yet acquainted with, as poſſeſſed of the power of abruptly cutting ſhort their courſe. We ſtill however perceive, that theſe diſeaſes have a general tendency to terminate in a given time and ſteadily to go through a regular progreſs, in ſpite of the greateſt exertions of art. If we review the practice which medical people have followed in fevers, from the days of Hippocrates to the preſent times, we meet with ſuch contradictory methods of treatment, as render

it impoſſible to avoid pronouncing, that if one man had actually ſaved life, another's endeavours ſeemed as if intended to deſtroy it: yet few authors have ventured to offer the fruits of their labours to the public, without previouſly boaſting more ſucceſsful methods of cure, than were known to their predeceſſors. Hence, if we are not ſometimes diſpoſed to doubt their veracity, we can hardly avoid concluding, that their practice had been feeble, and of ſmall effect. We lament, with reaſon, that medical facts are frequently of little value: nay, that they oftener miſlead, than guide us in the way to truth. An overfondneſs for ourſelves is, perhaps, more the cauſe of this, than real want of candour; the natural propenſity of the human mind to flatter itſelf, diſpoſing us to attribute cures to remedies, which were adminiſtered near the critical periods of the diſeaſe; while twenty inſtances, where ſimilar treatment produced no apparent effect, are inſenſibly blotted from the memory. This at leaſt was the caſe with myſelf. I flattered myſelf in many inſtances, that I had actually ſaved life:—I now find, on maturer reflection, that I had in reality done no material good. Thus it frequently happens, I believe, that practitioners boaſt of cures, to which they have no right; at the ſame time I am convinced, that they are frequently charged with deaths, of which they are innocent. The life of man does not appear to depend upon ſo ſmall a matter, in febrile diſeaſes, as is generally imagined; and is not often preſerved or endangered by the routine of common practice. It is not always eaſy to know exactly the real effects of treatment; neither has the road, which leads to this knowledge, been purſued with ſufficient induſtry. Fevers occur frequently, and on that very account, have been traced leſs minutely in their courſe, than ſome other diſeaſes. There are few practitioners, who write down in the preſence of the ſick, a minute and accurate hiſtory of the various caſes of fevers, which come under their care; who obſerve

carefully the changes which happen from day to day; who note the particular methods of cure, and the effects which arise in consequence of every alteration of treatment. Yet, unless all these circumstances are attentively considered, we cannot hope to form conclusions, which are in any degree to be depended upon. If we defer making remarks till the patient recovers or dies, difficulties will be easily got over, and such facts as contradict opinions, in which we have long believed, will be more easily reconciled, as being less perfectly remembered. Hence it is that a man may continue a very extensive practice, for a very long life, without ever once getting a view of the real truth.

As from what has been said before, little doubt can remain of the precariousness of trusting the cure of fevers to the simple efforts of nature; or, if we except intermittents, to the efficacy of particular remedies which cut off their course abruptly, it remains to look out for some other principle, which may serve us as a guide in the conduct of our practice. It is a truth to which we may accede without hesitation, that the cause of fever, as I have mentioned before, whatever it is, or however modified, has a general tendency to destroy the powers of life; while we likewise know, that there is a principle in the mechanism of the frame, which resists destruction to a certain degree. We also know, that the cause of the disease differs in degree of force; and that it is differently modified according to various circumstances; as we likewise observe, that the principle of life, or power of resistance, is different in activity, in the different parts of the body. To these two powers, viz. the force of the morbid cause and the powers of the constitution our views in practice must be principally directed. Hence we may establish a general rule, that wherever the force of the morbid cause is weak; at the same time that the powers of life in the general system, and particularly in the organs of vital importance, are

ſtrong and active, we have nothing more to do than to look on. On the contrary, where the cauſe of the diſeaſe is of unuſual force, or where the powers of life are preternaturally weak, our interference ought to be ſpeedy, bold and decided. In other words, we leave the buſineſs chiefly to nature, or take it entirely out of her hands. It is this which a practitioner muſt firſt determine, when called upon to give his aſſiſtance to a patient labouring under a fever. If the powers of the conſtitution appear to be equal to the taſk, our interference would be officious, and perhaps might prove hurtful. If aſſiſtance is neceſſary, it ought to be our principal aim to render it complete; but in proſecuting this view, we meet with much difficulty and impediment. We are not yet acquainted with any one remedy, which has a certain and infallible power of cutting ſhort fevers in the midſt of their courſe. It is not therefore abſolutely in our power to take the buſineſs entirely out of the hands of nature. We can, in fact, go no farther, than to oppoſe her pernicious efforts; or to obviate the fatal tendencies of the diſeaſe. The fatal tendencies of the diſeaſe, are variouſly modified, and the means by which they muſt be obviated, are ſometimes directly oppoſite. Two general modes, however, of the fatal action of fevers may in moſt inſtances be diſcovered. The cauſe of the diſeaſe, in one caſe, exerts its influence on the ſources of life and motion; in the other, the ſtructure of an organ of leſs importance is deſtroyed, and death happens only from a ſecondary effect. There is perhaps no fundamental difference in theſe different modes of action; yet the indications of cure, which ariſe from this view, are totally oppoſite. In the firſt inſtance, it is neceſſary to excite, and to ſupport the general powers of life: an indication of very great extent. In the other, it is ſometimes neceſſary to diminiſh the general reaction of the ſyſtem; to obviate irregular determination, and to oppoſe with vigour the tumultuary efforts of nature.

CHAP. X.

OF THE PARTICULAR CURE OF THE FEVER OF JAMAICA.

I SHALL begin this ſubject with obſerving, that the fever, which prevailed in the diſtrict of Savanna la Mar, was naturally a diſeaſe of the remitting kind; yet circumſtances were ſometimes connected with it, in ſuch a manner as prevented it from aſſuming its proper form. To remove thoſe circumſtances, which thus maſked or concealed the real genius of the diſeaſe, was conſidered as the firſt ſtep towards a cure. The accompliſhment of this purpoſe, however, was ſometimes difficult; neither could it always be effected by the ſame means. Thus it happened frequently in caſes, where there was exceſs of excitement, or a high degree of inflammatory diatheſis, that the remiſſions where ſcarcely perceptible; as it was likewiſe obſerved, that where there was a want of reaction, the paroxyſms were often languid and obſcure. In the one caſe, the remiſſions diſcovered themſelves in conſequence of bleeding, dilution and copious evacuation; in the other, wine and cordials determined the diſeaſe to aſſume its proper genuine form.

In the firſt place, evacuations were uſually employed as the means of procuring remiſſion, where the inflammatory diatheſis prevailed in exceſs; I may add, that they were proper for the moſt part, and that they ſeldom failed of producing the effect. Bleeding was frequently neceſſary, and generally of ſervice. Its efficacy, however, was often heightened by particular modes of management. Thus relaxation of ſpaſms, and

removal of inflammatory diathesis, more certainly followed bleeding, if the blood was drawn from a large orifice; if the patient was placed in an erect posture, during the operation; and more certainly still, if the lower extremities were at the same time immersed in warm water. When bleeding had been premised, and repeated according to the circumstances and urgency of the case, it was then customary to open the body freely: for which purpose, I have not found any thing answer better, than a thin solution of Glauber or Epsom Salts, with a small portion of emetic tartar. The operation of this medicine was extensive. It might be so managed as to occasion nausea, or moderate vomiting; to operate briskly downwards, or to promote a gentle diaphoresis. Remissions were generally the consequence of this method of proceeding, where there was no defect in the manner of conducting it. But where it it so happened, that the circumstances of the patient forbad the use of this laxative; or where it might not be proper to carry it to a sufficient length, benefit was derived from a powder, composed of nitre, camphire, emetic tartar and opium, given in pretty large doses, and repeated frequently. Remission, at least a great abatement in the violence of symptoms, was generally the consequence of this plan of treatment; particularly, if assisted by the plentiful dilution of watery liquors, by warm bathing and by large glysters of simple water. It is superfluous to mention the use of blisters in cases of local affection; but it will be less expected, that this remedy should be recommended in fevers, where there is an excess of the general inflammatory diathesis.—I can, however, bear testimony to its efficacy. The manner by which blisters produce their effects, is not yet agreed upon among authors; neither do I pretend to throw any new light upon the subject; but I would beg leave to suggest, that the mode of affording relief in the present, at least, did not seem to be much

unlike the effect of local affections, in consequence of which the violence of fevers is sometimes observed to subside.

I pursued the above method of procuring remission in those fevers, where there was real inflammatory diathesis prevailing in excess; but it so happened, that the signs of this diathesis were fallacious, appearing in some instances to be present, though the real genius of the disease was actually of a different nature; a circumstance, which occasioned a difference of management in conducting the method of cure. Excessive evacuations were not only unsafe in such cases, but in general had not any powerful effects in disposing the disease to assume a remitting form. Bleeding, however, was often found to be necessary, though it was seldom requisite to repeat the operation. The good effects which were observed to follow the use of cathartics, were not in general very remarkable; yet it was proper, in most instances, to open the body freely; for which purpose, no form of medicine, with which I am acquainted, answered better than a solution of salts with a small portion of emetic tartar, and sometimes with the addition of laudanum. In cases of local pain, blisters applied near the seat of affection were always of eminent service; and in cases of general irritability, they were often equally useful, when applied to the back part of the head and neck. A powder composed of nitre, camphire, emetic tartar and opium, was likewise employed with success; but the liberal use of warm bathing, was still more to be depended upon. No person, perhaps, will refuse consent to the method of proceeding, which I have hitherto recommended; but when I mention a free and bold use of cold bathing, even in an early stage of this fever, I do not expect the same concession. To dash cold water on the head and shoulders of a person in a fever, has an appearance of rashness and hazard. I can, however,

produce the teſtimony of repeated experience for the ſafety of the practice, no leſs than for its ſucceſs in procuring remiſſion; and ſhall therefore conſider it a duty to recommend it warmly to the public. Wherever it was employed,—and the caſes in which it was tried were numerous, a calm and equable perſpiration, additional tone and vigour, with great abatement of irritability, were conſtantly obſerved to enſue.

The paroxyſms and remiſſions were generally diſtinct in the beginning of fevers, where the nervous ſyſtem was principally affected; but often became leſs ſo, as the diſeaſe advanced in its progreſs; a circumſtance which did not ariſe oftener from the nature of the complaint, than from the common method of treatment. Bleeding was often diſpenſed with in the fevers of the Weſt-Indies; but vomiting and purging were indulged in with freedom. The diſtinction of paroxyſm and remiſſion was ſometimes evidently rendered obſcure by this practice; while it was likewiſe obviouſly reſtored again, by the uſe of wine and cordials, which excited the powers of life. In this ſpecies of diſeaſe, evacuations were ſeldom neceſſary; ſeldom indeed admiſſible in a great extent. Bleeding unleſs under particular circumſtances, was totally improper. Cathartics were ſometimes dangerous, and antimonial vomits often ſunk the patient irrecoverably. Bliſtering, on the contrary, even at an early period, was generally of ſervice; as alſo were opiates, and a judicious uſe of the warm bath; but cold bathing with ſalt water, was, of all others, the remedy of the moſt powerful effect. I do not pretend to ſay, that it abſolutely ſtopped the courſe of the fever; but I can ſay with truth, that it generally reſtored the diſtinction of paroxyſm and remiſſion, diminiſhed irritability, and imparted a degree of tone and vigour to the ſyſtem, which was juſtly conſidered as a ſign of ſafety.

To procure remiſſion in fever, diſtinguiſhed by a prevalence of the putreſcent tendency, is not in every

inſtance an eaſy taſk. A remitting fever, with marks of ſpecific putrefaction, is not a diſeaſe of common occurrence in Jamaica; but a fever with ſigns of putrefactive tendency, mixed with ſymptoms of great irritability, or a high degree of malignity, is not altogether rare. From the complicated nature of the diſorder, the indications of cure are often difficult and perplexed. Bleeding is univerſally condemned; more, I believe, from theory than from actual obſervation. It was, and perhaps ſtill is, a faſhionable mode of reaſoning, to impute the languors and other marks of debility, which are common in the fevers of the Weſt-Indies, to a putrefcent tendency in the ſyſtem. Such ſymptoms however are in fact more generally the attendants, or diſtinguiſhing ſigns of fevers, where the nervous ſyſtem is affected. In ſuch caſes, bleeding is obviouſly hurtful; in the one of which we now treat, (where ſuch a diſeaſe actually exiſts,) it is not only a remedy of ſafety, but of very eminent ſervice, previous to the application of cold, particularly previous to cold bathing, which may be uſed with freedom and boldneſs. Cold bathing, indeed, is the remedy on which we muſt principally depend. There are others which do good occaſionally; but this is the only one I know, which has any very conſiderable effect in changing the nature of the diſeaſe. There is a general rule in the practice of medicine, which requires to be particularly remembered in thoſe complicated ſpecies of fever, viz. that as the indications of cure are often embarraſſed; ſo the appearances, which principally point to danger, are firſt to be attended to; while the plan of cure, which we determine to be the moſt proper, muſt be followed up with vigour and reſolution. We ought always to bear in mind, that in dangerous and difficult caſes feeble remedies, or even powerful ones timidly uſed, are of little avail. Cold bathing, employed with timidity, failed of doing good in ſome inſtances. I

met with no example, where the boldeſt uſe of it did harm. It was ſeldom, I muſt again repeat, that it did not ſucceed in obviating irritability, in checking the putreſcent tendency, and in imparting to the ſyſtem that degree of tone and vigour, in which ſafety is obſerved to conſiſt.

The method of procuring remiſſion, in thoſe fevers which were diſtinguiſhed by local affections, or irregular determinations to particular parts, was nice, and ſometimes difficult. Bleeding was frequently proper, eſpecially, if there ſubſiſted at the ſame time marks of a general inflammatory diatheſis: but it was ſeldom ſufficient wholly to accompliſh the buſineſs. However, together with a judicious management of warm bathing, it greatly heightened the good effects of bliſters, the remedy on which the principal dependence was placed. In fevers which were accompanied with uncommon pain of the head, I have ſometimes found it ſerviceable to apply cold to the part affected; the feet being at the ſame time immerſed in warm water, and blood flowing by a large orifice from the arm. I alſo frequently obſerved, that the general fever ran higher, though it likewiſe more certainly aſſumed its proper form, in conſequence of bleeding, bliſtering, and the removal of local pain. In thoſe fevers, where bilious appearances were the effect of accidental, irregular determination to the ſtomach or liver, the remiſſions were often obſcure: neither did the method of treatment, which was generally adopted, ſeem to be well calculated to bring forth the natural, genuine appearance of the diſeaſe. Bilious appearances, it muſt be owned, ſometimes vaniſhed, while the type of the fever became more diſtinct after the exhibition of an emetic or briſk cathartic; yet there is cauſe to doubt if this depended on the evacuation of bile. It might be ſaid, with more truth, perhaps, that the action of the emetic, by exciting the powers of the ſtomach and biliary ſyſtem, effected a change in the irregular determina-

tion, which had formerly taken place to thoſe parts. It was generally obſerved, where good effects did not follow the firſt exhibition of remedies of this kind, that harm was uſually the conſequence of a ſecond. Vomiting, in ſhort, was often rendered continual, and the diſtinction of paroxyſm and remiſſion was apparently deſtroyed, in conſequence of the operation of violent emetics. Inſtructed by repeated examples of their hurtful effects, I at laſt ſcarcely ever employed antimonial vomits; even the ſafeſt kinds were uſed with caution. If it appeared, at any time, that the action of vomiting would be ſerviceable, camomile tea, or at fartheſt a few grains of ipecacuana were generally thought ſufficient for the purpoſe. When this buſineſs was finiſhed, a draught of cordial ſtimulating liquor, which had a tendency to promote a diaphoreſis, was next adminiſtered. By this mode of treatment, eſpecially if a bliſter was applied at the ſame time to the region of the liver, I have the ſatisfaction to add, that the bilious appearances for the moſt part vaniſhed, and, if care was taken to ſupport a determination to the ſurface, ſeldom ever returned during the continuance of the fever. Different ſeaſons, and different ſituations of country were particularly diſtinguiſhed by correſponding determinations. Thus a tendency to the bowels and biliary ſyſtem was chiefly remarkable in the autumnal months, and in low and champaign countries; the head and breaſt were oftener affected in the winter months, and in hilly ſituations.

I obſerved before, that it is the firſt object in the cure of fevers, to remove thoſe circumſtances, or accidental ſtates of the body, which hinder the diſeaſe from aſſuming its proper form. Thus, to procure remiſſion, appeared univerſally to be the firſt buſineſs in the cure of the fever of Jamaica; the next, and a very important one, is to prevent the return of the paroxyſm. If we knew a remedy, which

could be depended upon to accompliſh this purpoſe with certainty, the cure of the diſeaſe would be eaſy; but the Peruvian bark, which almoſt infallibly ſtops the courſe of intermitting fevers in all countries, does not ſeem ſo indiſputably to poſſeſs the ſame power over the uſual endemic of the Weſt Indies. I am ſorry to own, that my knowledge on this ſubject, is not altogether ſatisfactory. At the time I left Jamaica I did not entertain a doubt, that the endemic of the diſtrict, where I reſided, poſſeſſed ſomething in its own nature which decidedly determined its duration. It was uſually obſerved to terminate on a critical day, generally by very evident ſigns of criſis, and without ſeeming to be materially effected in its courſe, by the various and oppoſite modes of treatment which were ſometimes purſued. But though this was true for the moſt part, yet the ſuperior efficacy of the very large doſes of bark, which I afterwards ventured to give in the intermitting fevers of North America, obliges me to ſpeak with heſitation, when I mention the powers of that remedy. It is probable that bark, with the ſame management, might have had the ſame effects, in the fever which prevailed at Savanna la Mar, as in that which is commonly epidemic in Georgia and the Carolinas; yet no doubt remains, that as commonly employed in Jamaica, it has no right to be conſidered as a remedy, which abruptly cuts ſhort the courſe of the diſeaſe. In every caſe where it was tried, (except one) it did not ſeem to do more than give a degree of tone and vigour to the ſyſtem, to excite a certain ſtate of inflammatory diatheſis, in conſequence of which, the criſis was obſerved to be more perfect and complete, though it did not perhaps actually happen at an earlier period. Thus I am diſpoſed to conclude, from all the experience which I have had, that bark is not generally carried farther, in the cure of the remitting fever of Jamaica, than merely to ſupport the tone and vigour

of the powers of life. If we truſt to it for more, in doſes of two ſcruples or a drachm given every two hours, we ſhall certainly be deceived.

Having mentioned the different methods of treatment, by which it was attempted to procure remiſſion in the endemic fever of Jamaica, and having likewiſe endeavoured to aſcertain how far we can go in preventing the return of paroxyſms, it only remains to detail ſome particulars in the management of the plan of cure, where the different ſpecies of fevers were diſtinguiſhed by a peculiar train of ſymptoms. It was obſerved in general, that fevers, with a moderate degree of inflammatory diatheſis, ſeldom required our interference. The diſeaſe, after a certain duration, terminated uſually of its own accord. After I had gained ſome experience of the general courſe of fevers, I uſually allowed thoſe, in which I did not perceive marks of danger, to go on their own way, that I might better diſcover thoſe periods, at which the diſeaſe was naturally diſpoſed to terminate. Thus where the paroxyſms continued regular and diſtinct, the remiſſions perfect, and the vigour unimpaired, nothing material was attempted to be done. On the contrary, where the paroxyſms were long, or leſs diſtinctly formed, with ſigns which indicated an approaching affection of the nervous ſyſtem, bark, and other remedies, which excited and ſupported the powers of life, were given with the earlieſt opportunity. Changes from inflammatory diatheſis to nervous affection, were obſerved to happen frequently on the fifth day. Bark, and ſuch remedies as imparted tone and vigour to the ſyſtem, were given without delay; and the diſeaſe terminated for the moſt part on the ninth. In thoſe fevers, which were of a complicated nature, in which ſigns of inflammatory, nervous, or putrid diatheſis were variouſly mixed, bliſters applied in different manners, opiates, bathing and antiſpaſmodics were often materially uſeful; but it would

be arrogance to attempt to deſcribe rules for the particular mode of application, which muſt vary more or leſs in almoſt every caſe, and which only can be learned from actual obſervation. There is one rule, however, in the treatment of fevers, of which the practitioner ought never to loſe ſight, viz. that wherever it is neceſſary to interfere, it is only the moſt vigorous deciſion which can do good. We cannot, as was ſaid before, depend with certainty upon bark, as a remedy poſſeſſed of the power of abſolutely cutting ſhort the cauſe of the fever of Jamaica; yet wherever the fevers of that country diſcovered ſigns of nervous affection, I do not know any thing in the materia medica, from which ſuch beneficial effects may be expected. If it did not actually ſtop the diſeaſe, it was eminently ſerviceable in conducting it to a favourable iſſue. Opium, wine, ſnake-root, &c. were often obſerved to heighten its good qualities: but the particular uſe of ſuch additions can only be regulated by circumſtances. Wine has been freely recommended in fevers with ſymptoms of nervous affection; and it muſt be owned, that its good effects were conſiderable, not only in real debility, but wherever the cauſe of the diſeaſe acted by weakening or depreſſing the powers of life. Wine was likewiſe obſerved to be more uſeful in caſes of mobility and weakneſs, than in caſes of ſtupor and ſuſpenſion of the nervous influence. But though it is actually a remedy of great value, its virtues appear to have been greatly enhanced. In many inſtances it was not proper in any quantity; in ſome, it was only proper in a ſmall quantity, and in very few, perhaps, could we allow of the quantities which are given in common practice. At one time I carried the uſe of wine in the nervous fever of Jamaica, to a very great length; but I afterwards learnt, that a third part of the quantity would have probably anſwered the purpoſe better. Though it undoubtedly is an uſeful cordial and tonic,

it is ſtill inferior to cool air, and particularly to cold bathing.

In thoſe treacherous and malignant fevers, which I have deſcribed in the third ſection of the ſixth chapter, the ordinary medical aids were often feeble and inſufficient. The courſe of the diſeaſe was generally rapid. There was little time left for deliberation; and where the firſt, at leaſt where the ſecond remiſſion paſſed over, without ſome bold and decided ſteps to prevent the return of the paroxyſm, or change the nature of the ſymptoms, the opportunity was probably never again in our power. The type of this malignant fever was uſually of the ſingle tertian kind; yet it was often found to anticipate, by ſuch long anticipations, that the ſecond paroxyſm ſometimes made its appearance, before any decided ſteps were taken by the practitioner to ſtop its progreſs, or often, indeed, before there were any ſurmiſes of danger. As this diſeaſe ſeemed to have a nearer reſemblance to the intermittent, than ſome other ſpecies of the endemic of Jamaica; ſo bark appeared to be capable of producing more effect, in the preſent inſtance, than in thoſe caſes where the remiſſions were more obſcure. It ſtill, however, deſerves to be remarked, that ſuch ſcanty doſes, as were uſually given in Jamaica, could not be effectual; indeed, we could ſcarcely hope, that any powerful alteration could ariſe from leſs than half an ounce given at the ſhort interval of every other hour. I confeſs candidly, that I never ventured ſo far; yet I alſo confeſs, that I am by no means ſatisfied with the ſucceſs of the method of cure, which I adopted at firſt, in this diſeaſe. I loſt ſome patients before I was aware of danger, and perhaps ſuffered others to die, from a dread of ſtepping over the bounds of common practice. At laſt, I acted with more deciſion; and have cauſe to be ſatisfied with the ſucceſs of the attempt. As ſoon as I diſcovered the malignity of the diſeaſe, the marks of which, for the moſt

part, were plain in the courſe of the firſt paroxyſm, the head was immediately ſhaved, and covered with a bliſter, which reached half way down the neck; the feet were likewiſe put into warm water, previous to the expected return of the paroxyſm; the body was rubbed with brandy or rum; wine, and ſometimes opium were given in quantity ſufficient to exhilirate the ſpirits, or to produce a low degree of intoxication; bark was alſo preſcribed in larger doſes than uſual, during the remiſſions; and cold bathing was occaſionally employed with freedom. I do not ſay, that the return of the paroxyſm was abſolutely prevented by this method of treatment, but I have the ſatisfaction to add, that the fatal tendency of the diſeaſe was evidently obviated by it.

I muſt farther obſerve, that bark has been conſidered as the principal remedy in thoſe fevers, the nature of which has been believed to be putrid; but the great fame of this remedy has proceeded from theory, rather than from actual obſervation. A real putrid diſeaſe, (as I have ſaid before), if we except the yellow fever, occurs very ſeldom in Jamaica. Symptoms of languor and debility, however, are frequent in the fevers of that country; appearances which, though in fact, only ſigns of nervous affection, have often been imputed to a putrid tendency in the habit. Bark has been found to be efficacious in thoſe caſes of languor and debility, which have been falſely thought to ariſe from a putrefactive tendency in the ſyſtem. Hence this remedy has gained credit, on a foundation which does not exiſt. I may add with truth, that the power of the bark was very equivocal, in thoſe caſes where the marks of putrid tendency were obvious. The principal dependence was then conſtantly placed in the cold ſalt-water bath; which, if well managed, produced moſt aſtoniſhing effects. An additional quantity of ſalt was often added to the water of the ſea; and care was taken

that the coldneſs ſhould be as great as the circumſtances of the climate would permit. The chief dependence I have ſaid, was conſtantly placed in this remedy, where the putrid tendency prevailed in the general ſyſtem; but where it was more particularly confined to the bounds of the alimentary canal, ſaline draughts, in the ſtate of effervescence, were uſeful, as were likewiſe glyſters of cold water impregnated with fixed air. It is ſuperfluous to mention cool air, clean linen, cold drink and the liberal uſe of wine.

The remedies, which I have hitherto mentioned, are ſuch, as ſeem chiefly intended to ſupport and invigorate the general powers of life; but which are leſs immediately directed to obviate the fatal tendency of particular ſymptoms; which I propoſed to conſider, as the ſecond indication of cure. I ſhall probably depart materially from the uſual ideas of practitioners in the proſecution of this ſubject. I do not deny, that bark may be given with ſafety in fevers, which are accompanied with local affections, or irregular determinations to particular parts; yet I muſt likewiſe obſerve, that bark is not the remedy on which the weight of the cure depends. Local affections, or irregular determinations are often diſtinguiſhed by the name of the efforts of nature. I do not indeed diſpute, where the part of the body, to which the determination takes place, is of little importance to life, or more certainly, where it is an organ of excretion, that ſuch parts as are of greater conſequence, are, in ſome degree relieved in conſequence of this effect; and though there is ſtill an impropriety, there is leſs danger in conſidering this irregular action of the morbid cauſe, as an effort of nature, or a quality of the vis naturæ medicatrix, by means of which, the health of the ſyſtem is eventually rendered more ſecure. This, however, is only a circumſtance of accident. It does not appear to depend upon a regular deſign of nature, and cannot

be admitted with ſafety as the baſis of a plan of cure. We find, in ſhort, that though the life of the whole is ſometimes preſerved by it, the deſtruction or derangement of the part is generally the conſequence. But that the fallacious tendency of thoſe tumultuary efforts of nature may be more clearly illuſtrated, I ſhall adduce the example of fevers, diſtinguiſhed by an increaſed ſecretion of bile. It is an opinion, which ſeems to date its origin from Hippocrates, that bile vitiated in quality, or redundant in quantity, deſerves to be conſidered as the cauſe of the ſpecies of diſeaſe diſtinguiſhed by the name of ardent fever: and it muſt be confeſſed, that the frequent appearance of bilious diſcharges, in the fevers of hot climates, gives countenance to the ſuppoſition. From the frequency of this ſymptom, perhaps, the practitioners of the Weſt-Indies adopted the idea, that bile is the cauſe of the fever of that country; while the method of cure, which they uſually purſue, has ſerved to confirm them in their error. Prepoſſeſſed with an opinion of the prevalence of bile, they adminiſter cathartics and emetics with a liberal hand. If bile appears in the firſt evacuations, they conſider it ſufficient authority to proceed; if it does not appear, they conclude that the remedy has not been of ſufficient force to reach the ſeat of the diſeaſe; and therefore perſiſt in their intentions, till the effect is at laſt produced. It is well known, that a repetition of cathartics and emetics ſeldom fails to produce the appearances of a bilious diſeaſe. Hence this ſymptom of fever, and all the dangers which follow it, are frequently the work of our own hands. That this is the caſe, appears from a relation of the method of cure, which I uſually adopted in ſuch fevers as were diſtinguiſhed by ſymptoms of this nature at an early period. Inſtead of encouraging the vomiting, or promoting the evacuation of bile downwards, I generally did every thing in my power to moderate, or even to check it.—

Sometimes I prescribed an emetic; but it was more with a view to excite the action of the stomach and biliary system, than to promote an evacuation of redundant or vitiated humours. After the operation of vomiting was finished, a blister was usually applied to the region of the liver, and such a plan of cure was pursued, as supported a determination to the surface of the body, and gave tone and vigour to the stomach and general powers of life. By this mode of treatment, bilious appearances vanished speedily, or ceased to be troublesome; while by the repeated use of emetics and cathartics, they generally continued long, and often prevailed throughout the course of the disease. We may thus, I hope, conclude, without any unnatural inference, that there appears to be danger in encouraging those tumults, which have been usually considered as the efforts of nature. If they are in fact efforts, no person can pretend to deny, that they are generally precarious; nay, that they are often the immediate causes of death.

I have now detailed the particular steps of the method of cure, which I adopted in the remitting fever of Jamaica; a disease which I treated, in some respects, on a different plan, and if self-love hath not blinded me with more success than the generality of those practitioners whom I had the opportunity of knowing, I treated the disease with success; but I dare not affirm, as some have done, that under this method of treatment, I never lost a patient. I proceeded, indeed, with diffidence and distrust of the powers of the medical art; venturing no farther than to support the general powers of life, and to obviate symptoms of a fatal tendency. Many pretend to cut short the course of fevers, by the force of a single remedy; but the means do not appear very obvious, and the effect was often precarious. I grant, that it is sometimes in the power of the practitioner to exterminate the cause of disease by forcible means, or

to deſtroy a certain aptitude of conſtitution, in which this diſeaſe may be ſaid to conſiſt; but I muſt at the ſame time obſerve, that there is danger likewiſe, leaſt he extinguiſh life. The bark, which has been ſo much celebrated for checking the courſe of fevers, though generally ſafe, is ſeldom effectual: others are frequently dangerous. During the time that I remained in the Weſt Indies, I obſerved attentively the ſtate of body, which uſually attended recovery; as likewiſe thoſe appearances which preceded, and apparently were the cauſes of death. Tone and vigour, or a moderate degree of the ſtate of body diſtinguiſhed by the name of inflammatory diatheſis, without local affection, afforded the ſureſt ſigns of ſafety; general failure of the powers of life, or irregular determinations to organs of importance, were the moſt certain appearances of danger. Thus, after obviating particular ſymptoms of a fatal tendency, it was the principal indication to ſupport the general powers of life, or to excite the tone and vigour of the ſyſtem. This was beſt accompliſhed by bark, wine, cool air, and above all, by cold bathing, which I am induced to conſider as the moſt important remedy in the cure of the fevers of the Weſt-Indies; and, perhaps, in the cure of the fevers of all hot climates. Though it might not abſolutely cut ſhort the courſe of the diſeaſe; yet it ſeldom failed to change the fatal tendency of its nature.

CHAP. XI.

YELLOW FEVER. (a)

THE diſeaſe, known in the Weſt Indies by the name of yellow fever, particularly that ſpecies of the diſeaſe diſtinguiſhed by black vomiting, has not, ſo far as I know, been deſcribed by the practitioners of any other country. In the autumnal fevers of moſt climates, indeed, as well as in the remitting fever of Jamaica, yellowneſs is not by any means uncommon: neither is vomiting of matter of a dark colour altogether rare, in the moments preceding death: yet in as much as I can judge, from what I have ſeen myſelf, or heard from others, the complaint which is the ſubject of the preſent treatiſe, poſſeſſes ſome characteriſtics of its own, different from thoſe of every other diſeaſe. I do not pretend to determine, in what this characteriſtic difference preciſely conſiſts: yet I may ſay with confidence, that the ſpecies of this diſeaſe, which terminates in black vomiting, may be diſtinguiſhed with certainty from the autumnal fever of aguiſh countries, or from the endemic remitting fever of Jamaica, even in the firſt hours of its attack. I muſt, however, at the ſame time own, that there is a ſpecies of diſeaſe, where the remiſſions are obſcure, and where ſigns of nervous affection, or even ſometimes of apparent inflammation, are more conſpicuous than ſymptoms of putreſcency, that I have ſome difficulty in claſſing properly. There is not any thing more foreign to my intention than multiplying names, or eſtabliſhing diſtinctions which do not exiſt in reality; yet, as I have often ſeen inſtances of fever, to which yellowneſs and black vomiting did not ſeem to be eſſential; but in which paroxyſms and remiſſions were extremely obſcure,

or perhaps did not exist, I at last ventured to conclude, that this species of illness had more affinity with the disorder usually known by the name of yellow fever, than with the common remitting endemic of the country. I shall therefore describe it as a species of that disease; though I am less solicitous about fixing its place in nosological arrangement, than of giving a history of it, by which it may be easily recognized.

I am not ignorant that there are several authors, who have written on the subject of the yellow fever; yet I had not the opportunity of consulting any of them, except Dr. Hillary, during the time that I lived in Jamaica. This writer's method of cure was adopted very generally by the medical people of the island, and many of them were disposed to think favourably of its success. I cannot however conceal, that I was disappointed in every instance where I saw it employed. If, in any case, the patient recovered, this fortunate event appeared to be more owing to great natural strength of constitution, or to a lower degree of disease, than to the efficacy of the method of treatment. But besides, that this author's method of cure is feeble and ineffectual, the historical part is particularly defective; the disease, in short, being only very imperfectly discriminated from the common endemic of that country. The consideration of these circumstances, induced me to throw together the observations which I had made on this subject, during the time that I resided in the West Indies; and though I am conscious that they are imperfect, I still hope that they contain some hints which may be useful to those, who have had little experience of the fevers of hot climates.

In our enquiries into the history of the yellow fever, some circumstances present themselves to our observation which are not a little curious. It has never been observed that a negro, immediately from

the coaſt of Africa, has been attacked with this diſeaſe; neither have Creoles, who have lived conſtantly in their native country, ever been known to ſuffer from it: yet Creoles or Africans, who have travelled to Europe, or the higher latitudes of America, are not by any means exempted from it, when they return to the iſlands of the Weſt Indies. Europeans, males particularly, ſuffer from it ſoon after their arrival in the tropical countries; yet, after the natives of Europe have remained for a year or two in thoſe hot climates, eſpecially after they have experienced the ordinary endemic of the country, the appearance of the yellow fever is obſerved to be only a rare occurrence. But beſides, that this diſeaſe ſeldom diſcovers itſelf among thoſe people, who have lived any length of time in a tropical country, it has likewiſe ſcarcely ever been known to attack the ſame perſon twice, unleſs accidentally after his return from a colder region. The remitting fever, on the contrary, does not ceaſe to attack ſuch as have reſided the greateſt part of their life in thoſe climates; or who have lived after the moſt regular and abſtemious manner; a fact which ſeems to prove, that there actually exiſts ſome eſſential difference between the two diſeaſes; or which ſhews, at leaſt, that the revolution of a ſeaſon or two deſtroys in the European conſtitution, a certain aptitude or diſpoſition for the one diſeaſe, which it ſtill retains for the other.

Having thus premiſed ſome circumſtances, which regard the general nature of the yellow fever, I ſhall proceed to give a more particular deſcription of the diſeaſe, previouſly dividing it into three forms, in each of which, I believe, I have frequently ſeen it appear.

1. Into a ſpecies of diſeaſe, in which ſigns of putrefaction are evident at a very early ſtage, which is generally rapid in its courſe, and which uſually terminates in black vomiting. Yellowneſs ſeldom or never fails to make its appearance in the preſent in-

ſtance; and perhaps it is only this form, which, ſtrictly ſpeaking, can be called the yellow fever. 2. Into a form of fever, which either has no remiſſions, or remiſſions which are ſcarcely perceptible; in which ſigns of nervous affection are more obvious than ſymptoms of putreſcency; and in which yellowneſs and black vomiting are rare occurrences. 3. Into another form, in which regular paroxyſms and remiſſions cannot be traced; but in which there are marks of violent irritation, and appearances of inflammatory diatheſis in the earlier ſtage, which give way after a ſhort continuance to ſigns of debility and putreſcency, to which yellowneſs frequently ſucceeds, or even ſometimes the ſo much dreaded vomiting of matter of a dark colour. The diſeaſe, which I have divided in the above manner into three diſtinct forms, appears to be in reality only one and the ſame. The difference of the ſymptoms probably ariſes from very trivial or very accidental cauſes; and it is a matter of great difficulty to diſcriminate thoſe ſigns, which are eſſential and neceſſary to its exiſtence. It is in ſome degree peculiar to ſtrangers from colder regions ſoon after their arrival in the Weſt Indies, and may generally be diſtinguiſhed from the remitting endemic of the country, not only by the obſcureneſs, or total want of paroxyms and remiſſions, but likewiſe by a certain expreſſion of the eye and countenance, with ſomething unuſually diſagreeable in the feelings, of which words convey only an imperfect idea.

SECTION I.

I SHALL deſcribe, in the firſt place, the moſt common and moſt formidable ſpecies of this diſeaſe, which, as I obſerved before, is diſtinguiſhed by early ſigns of putreſcency, by an intenſe degree

of yellowneſs; and, towards its termination, uſually by vomiting of matter of a dark colour. It was mentioned in the preceding treatiſe, concerning the remitting fever of Jamaica, that fevers of different types had their different hours of invaſion; but no ſuch property was obſerved in the preſent diſeaſe. In ſome inſtances the yellow fever began in the morning, though the evening, upon the whole, was the more uſual time of its attack. The firſt ſymptoms were languor, debility and head-ach, together with an affection of the ſtomach peculiarly diſagreeable. This laſt often preceded the others, and was in ſome meaſure characteriſtic; but it is impoſſible to give a clear idea of it in words:—anxiety, nauſea, and certain unuſual feelings were ſo ſtrangely combined, that any deſcription, which I might attempt to give of this complicated ſenſation, would hardly be intelligible. The horror and ſhivering, which ſo uſually precede fevers, was ſeldom great in degree in the preſent inſtance; but it ſometime continued long, and was often accompanied with ſenſations of a very unpleaſant kind. The heat of the body, though rarely intenſe, was frequently of an acrid and pungent nature. The pulſe was weak and confined in its ſtroke. It was likewiſe frequent, and the nature of the arterial pulſations were creeping or vermicular; in ſhort there was a perpetual motion under the finger, combined with ſomething, which gave the idea, that the diſeaſe was not of the kind which has paroxyſms and remiſſions. Together with this, the eye was ſad and watery; or in ſome degree inflamed, having much that appearance, which is the conſequence of expoſure to the ſmoke of green wood. The face was often fluſhed; yet the fluſhing, in the preſent caſe, was different from that which ariſes from ordinary cauſes. There was a degree of confuſion, and often a degree of grimneſs joined with it, difficult to be deſcribed in words; but which a perſon, ac-

quainted with the appearances of the diſeaſe, immediately recognizes as a diſtinguiſhing mark of its character. The tongue was often moiſt, and generally foul; the thirſt was ſeldom great, and though there was uſually a peculiar nauſea, there was rarely any ſevere vomiting or retching. The breathing was hurried, for the moſt part, with much anxiety and diſtreſs; while the patient frequently expreſſed ſufferings, which a perſon, unacquainted with the nature of the diſeaſe, would be diſpoſed to believe were not real.

The ſymptoms, which I have enumerated above, are thoſe which uſually ſhew themſelves in the firſt twelve hours of the diſeaſe. I marked them with all the attention of which I was capable; yet ſtill I am fearful, that the hiſtory may not be ſo explicit as to be totally free from ambiguity. The characteriſtic marks of the yellow fever, are not by any means doubtful to a perſon well acquainted with the diſeaſes of hot climates; but they are not eaſily conveyed in words, and may often be overlooked by thoſe, who have drawn their information from books alone. I am induced to think ſo, from an inſtance which happened to myſelf. I had read Hillary's account of the yellow fever, both before and ſoon after my arrival in Jamaica; I had likewiſe heard ſome converſation on the ſubject, ſo that I might be ſuppoſed to have been tolerably well informed of the general character of the diſeaſe; yet the firſt perſon, who came under my care in this illneſs, was within a few hours of death before I knew the diſorder, or even ſuſpected it to be of a dangerous nature. Fortunately for the peace of my conſcience, the patient had been viſited, on both the firſt and ſecond day after the attack, by a practitioner who had lived many years in the iſland; but, between careleſſneſs and inexperience, the poor man's ſituation was either not known, or not attended to, till approaches of death were viſible. The body

had been evacuated very plentifully by a ſolution of ſalts, during the two firſt days of the illneſs; but no material good ſeemed to enſue from it. The patient complained ſtill more on the ſecond day, than he had done on the firſt; but as the external ſigns of fever were moderate, I really ſuſpected that he complained without much cauſe. It ſo happened, that I could not viſit him on the third; and on the morning of the fourth, he became of a deep orange colour, and vomited black matter in great quantity. I then ſuſpected, that this complaint, to which I had not paid particular attention, was actually the diſeaſe known by the name of yellow fever; but it appeared likewiſe, to be ſo far advanced in its progreſs, that I could do nothing more than witneſs the approach of death. My want of diſcernment, and in ſome degree my careleſſneſs, a charge from which I cannot altogether acquit myſelf, made ſo deep an impreſſion on my mind, that I turned over every circumſtance of the diſeaſe with which my memory ſupplied me; and I ſoon had that ſatisfaction to find, that the miſtake which I had committed, had not happened to me without leaving an uſeful leſſon. In ten or twelve days another perſon was affected in a manner ſo ſimilar to the former, that I ſuſpected the diſeaſe to be the ſame, and the event proved my ſuſpicions to have been well founded. From that time forward, I never found difficulty in diſtinguiſhing this particular form of fever, in the firſt hours of its attack, not only from the remitting endemic of the country, but even from the other two ſpecies of this diſeaſe, which I ſhall afterwards deſcribe.

A trifling abatement of the ſymptoms, was ſometimes taken notice of, in ten or twelve hours after the commencement of this diſeaſe; but in no inſtance ſo far as I have obſerved, was there ever ſo much alleviation, as with any juſtice could be called a remiſſion. The ſymptoms of diſtreſs, where any abate-

ment had been perceptible, recurred in a ſhort time with aggravation; and if there actually ever was any relief afterwards, it was only momentary and uncertain. The appearance of the eye became ſtill more deſponding, with a ſenſation of burning heat, and greater marks of inflammation, affording inconteſtible ſigns of the real genius and nature of the fever. The pain of the head was now violent; the countenance was confuſed and grim: the gums were frequently ſpongy, and diſpoſed to bleed; the tongue, which was ſometimes moiſt, ſometimes dry, was almoſt always foul; the thirſt was irregular; at one time intenſe, at another very little increaſed beyond what it naturally is. Nauſea, I obſerved above, was a common ſign, from the firſt hours of the illneſs; yet vomiting, during the firſt day or two, was not by any means a conſtant, perhaps ſcarcely a frequent ſymptom: neither, if it did take place, was it often found to be bilious. The liquor thrown up, for the moſt part, was clear; in ſhort, ſeldom altered from the ſtate in which it had been drank, unleſs by having acquired an unuſual degree of ropineſs, or by preſenting ſome flakes of a darker coloured mucus. To the above ſymptoms we might add, uncommon reſtleſſneſs and anxiety; a torment ſcarcely to be expreſſed in words; watchfulneſs; a hurried and difficult reſpiration; frequent deep and heavy ſighing, with more or leſs diſpoſition to faint, where any exertion was attempted. It deſerves to be remarked, however, with regard to the diſpoſition to faint in the yellow fever, that it did not depend upon the ſame cauſe, as in ſome other diſeaſes. It ſeemed, in fact, to be owing to torpor of the nervous power, rather than to exceſs of mobility. The patient was often able to ſtand upright, for ſome time, even to walk to a conſiderable diſtance; and when at laſt overcome, was obſerved to fall down in a torpid, rather than in a fainting ſtate. Sweating was a rare occurrence in

this ſtage of the diſeaſe; at leaſt I do not find, that I ever had remarked any greater degree of it, than a clammy moiſture on the head and neck. It was alſo rarely obſerved, that the external heat was much increaſed beyond its natural ſtate; while the pulſe now began gradually to abate in point of frequency. The yellowneſs, which is intenſe in the laſt ſtage of the diſeaſe, was ſeldom ſeen in the period which I now deſcribe; yet, together with a general obfuſcation of countenance, a tawney hue rather than a paleneſs, was obſerved about the eyes and corners of the mouth, when the patient turned accidentally languid and faint. The body was frequently coſtive in the firſt days of the illneſs; and I have even ſeen ſome inſtances where ſtrong cathartics did not occaſion the uſual evacuations. The urine was generally high coloured, and turbid. In ſome caſes there was active hœmorrhage from the noſe; which was generally followed with ſome relief from the violent pain of the head. I have alſo obſerved a high degree of delirium, though I never ſaw any inſtances, where this ſymptom was of long continuance.

The duration of the tumultuary ſtate, which I have deſcribed is uncertain. Sometimes it did not exceed twenty-four hours, though it more generally continued till the third day; ſometimes even longer. The ſymptoms, which now made their appearance, were many of them different in their nature from the former. The agony of diſtreſs, which was ſo ſtrongly depicted in the countenance of the patient, during the firſt days of the diſeaſe, was obſerved about this time to be ſenſibly diminiſhed; the eye became more cheerful, the countenance more ſerene and compoſed; yet yellowneſs of the ſkin became ſpeedily evident: the external heat and fever ſubſided; the pulſe became gradually fuller and ſlower, and approached by degrees to its natural ſtate: no ſweat or moiſture was now obſervable on any part of the body; the ſtate of

the skin impressed the idea, as if it were not pervious to any degree of perspiration, and heat gradually forsook the surface and extremities; the tongue turned moist, and at the same time frequently clean about the edges: the gums turned redder, more spongy, and shewed a greater disposition to bleed: vomiting was now troublesome—the liquor thrown up was ropy, much in quantity, and abounding with villous or mucus flakes of a darker colour: thirst in a great measure vanished; but sensations of anxiety, distress and uneasiness in the region of the stomach suffered no material abatement.

Things went on in this manner, sometimes for one day only, sometimes for two, three or more. The circulation in the extreme vessels became gradually more languid; the natural heat retired from the surface of the body, which was now dry and impervious; the pulse returned nearly to its ordinary state, or became slow, full, and regular; the yellowness increased fast; so that the whole of the body was frequently yellow as an orange, or of as deep a colour as the skin of an American savage; anxiety was inexpressible; vomiting was irrestrainable, and the so much dreaded symptom of vomiting of a matter resembling the grounds of coffee, at last made its appearance. It deserves, however, to be remarked, with regard to this formidable symptom, that the colour of what was thrown up, was often black as soot, where the disease had hurried on rapidly to the last stage: while it was not only less intensely black, but was often tinged with green, where the progress had been slow and gradual. I observed before, that villous or mucus flakes were discovered early in the vomitings of the patient, and that these appearances increased as the disease advanced in its progress. I may now add, that streaks of blood were sometimes found to be joined with them; the greatest part of which seemed to come from the throat and gums. The

vomiting, which now returned at ſhorter intervals as the diſeaſe approached this fatal period, was ſeldom accompanied with violent retching. A quantity of liquor, ſometimes a quantity ſo enormous, that we could not help wondering whence it had been ſupplied, having been collected in the ſtomach, was diſcharged without much difficulty, and the patient enjoyed ſome reſpite till a like accumulation had again taken place. It may further be remarked, that as ſoon as the vomited liquor acquired this dark and ſooty colour, the belly generally turned looſe, the ſtools being black and ſmooth, not unlike tar or molaſſes. The tongue likewiſe became clean, the gums became putrid; hœmorrhage, or rather oozings of blood were ſometimes obſerved at different parts of the body; while livid blotches frequently made their appearance on the belly and inſides of the thighs. The pulſe, which during the latter ſtages of the diſeaſe, could ſcarcely be diſtinguiſhed from the pulſe of a perſon in health, became at laſt quick, irregular, or intermitting; ſoon after which, coma or convulſions cloſed the ſcene. It may not be improper to remark, before leaving the ſubject, that the yellowneſs of the ſkin, which was ſaid to precede the black vomiting in moſt inſtances, in ſome caſes was found to ſucceed to it. In ſuch, the vomiting began unexpectedly, or without much previous affection of the ſtomach; the colour of it was uſually intenſely black, the patient turned yellow almoſt in an inſtant, and died in a very ſhort ſpace of time:—the diſeaſe, in ſhort, paſſed ſuddenly from the firſt ſtage to the laſt.

I may obſerve in this place, that the number of thoſe who recovered from the laſt ſtage of this ſpecies of the yellow fever, was extremely ſmall: yet, though ſuch fortunate inſtances were rare, they were not altogether wanting. The termination, however, did not appear to be by regular criſis. The black vomiting ceaſed, ſometimes apparently in conſequence

of treatment, ſometimes evidently of its own accord; but a vomiting of a ropy, glutinous matter continued for a great length of time, together with an extreme irritability of ſtomach, and a very peculiar ſtate of the ſkin; which ſometimes did not recover its natural ſmoothneſs and unctuoſity, till after ſeveral weeks had elapſed.

During the time that I lived in Jamaica, I opened ſeveral perſons who died of this diſeaſe; but it was ſeldom that I found any material variation in the appearances. Soon after death, and even ſometimes before death had actually taken place, the body became covered with large livid blotches; and, it is almoſt unneceſſary to mention, was extremely offenſive. In opening the abdomen, the omentum and all its appendages were diſcovered to be in a dry and parched ſtate, and of an uncommon dark grey colour. But together with this dark grey colour of the omentum, and a want of the unctuoſity or moiſture, which is uſually found in the cavity of the abdomen, the ſtomach and inteſtines had a dirty yellow appearance, were highly putrified, and much diſtended with wind. The liver and ſpleen were generally enlarged in ſize; the colour of the liver was often of a deeper yellow, than that of any other of the abdominal viſcera; while the texture of the ſpleen was frequently leſs firm, than it is found to be in its natural ſtate. The gall-bladder, for the moſt part, was moderately full; but the bile it contained, was black and thick, not unlike tar or molaſſes. The biliary ducts were likewiſe enlarged, and moderately filled with the ſame ſort of bile, which was found in the gall-bladder: while the very blood veſſels of the liver bore the marks of uncommon diſtenſion. In the cavity of the ſtomach likewiſe, there was uſually more or leſs of a dark coloured liquor, ſimilar to what had been thrown up in the laſt ſtage of the illneſs. But beſides, that this dirty fluid was generally preſent in the ſtomach

in conſiderable quantity, the villous or inner coat of that organ was alſo abraded in various places; at the ſame time that ſome ſpots appeared on the ſurface, which were probably the beginnings of mortifications. The ſuperior portions of the inteſtinal canal were generally in a ſituation ſimilar to what I have deſcribed; only it muſt be remembered, that the morbid appearances were not yet ſo far advanced in progreſs.

The ſtate of the body, as it appeared on diſſection, throws conſiderable light on the nature of the yellow fever. It enables us to explain ſatisfactorily many of its leading ſymptoms; and may even afford uſeful hints in the conduct of the cure. It was mentioned above, that the natural heat and vigour of circulation retired from the ſurface and extremities of the body at a certain period of the diſeaſe; and that a copious and obſtinate vomiting enſued ſoon after this change had taken place. The fluid thrown up, which was uſually pituitous, glutinous, or flakey in the beginning, acquired, after ſome time, a colour of various degrees of blackneſs. In quantity, it was often immoderate, bearing no proportion to the liquor which was drank; a circumſtance which could only be explained by the ordinary determination to the ſurface of the body being turned upon the internal parts; in conſequence of which, there was a preternatural diſcharge of fluid into the cavity of the alimentary canal. Flakes, of a mucus or villous nature, were likewiſe frequently obſerved in thoſe matters which were thrown up by the patient; an appearance which we could not have eaſily accounted for; unleſs we had diſcovered, in examining the dead body, that the inner coat of the ſtomach was actually abraded; but in what manner this happened, may be difficult to explain. It might either ariſe from the repeated action of ſevere vomiting; or, ſtill more probably, from the preternatural and forcible determination to the exhaling veſſels of this cavity, forcing

off some portions of the villous coat, in the manner of cuticular blisters. To which explanations I may add, that the black colour of the vomited matter, was evidently owing to a mixture of vitiated bile; the passage of which might be easily traced from the gall duct into the pylorus.

The species of the yellow fever, which I have now described, is universally acknowledged to be a terrible disease; and there are few, I believe, so uncandid, as to boast of general success in the manner of curing it. A road is therefore left open, not only for improvement, but almost for total innovation. The only author I have read on the subject, or the practitioners with whom I am acquainted, do not seem to have extended their views beyond the symptoms of the disease. There are some, who, from observing that there is pain of the head and flushing of the face, recommend bleeding; others, from the presence of nausea or inclination to vomit, make trial of emetics; and many, from various causes, insist on the indispensible use of cathartics. My views, I must confess, are different from those of preceding authors. Bleeding was employed occasionally; emetics were cautiously avoided; but time appeared to be too precious to be spent in attending to the effects of cathartics, which cannot often be known in less than twenty-four hours; and which at best are precarious or feeble. Instead, therefore, of attempting to evacuate redundant bile, or to correct it when supposed to be vitiated, I exerted myself, from the first moment that I was called to the patient, to change the genius and natural tendency of the disease; or, if I may be allowed the expression, to take the business, as speedily as possible, totally out of the hands of nature.

I remark in the first place, that I generally began the cure of this species of the yellow fever with bleeding. Bleeding was employed in the present case, chiefly with a view of paving the way to remedies of

greater efficacy. It was, however, found to moderate the violence of local pain, particularly the violence of the head-ach, and to be not altogether without effect, in retarding the usual rapid progress of the disease. It has hitherto been thought necessary, indeed almost indispensible, to empty the first passages in this species of fever; but time is short, and the good which accrues from such evacuations, is not very certain, and often not essential. It was, therefore, thought sufficient to trust this intention, for the most part, to laxative glysters; after the employment of which, (bleeding having been premised in such quantity as was deemed proper,) the patient was washed clean, and bathed in warm water, in as complete a manner as the circumstances of situation would permit. It is needless to mention, that this was done with a view to increase mobility of system, and to remove spasmodic stricture from the extreme vessels of the surface; in consequence of which, greater benefit was expected from the application of cold salt-water, which was dashed suddenly from a bucket on the head and shoulders. This practice may appear hazardous, to those who argue without experience; but I can vouch for its general safety, and bear testimony to its good effects. Sweat, with perfect relief from all the feelings of anxiety and distress, was generally the consequence of this mode of treatment. If employed within the first twelve hours from the attack, it seldom failed of removing all the symptoms of danger: or of effecting a total and complete change in the nature and circumstances of the disease; but if the progress was more advanced, though the same rule of practice might still be proper, the execution required more boldness and decision. It is only possible to judge from the circumstances of the case, at this period, of the necessity or propriety of bleeding, and of emptying the lower intestines by means of glysters; but when this business shall have been ac-

compliſhed, in ſuch manner as may be deemed right, or conducive to the main view, it will be adviſeable to ſhave the head, to bathe the whole body in warm water, and inſtantly to daſh cold water from a bucket on the head and ſhoulders. I have even ſometimes, where there was an appearance of greater obſtinacy, ventured to wrap the whole body in a blanket ſoaked in ſea water, or water in which was diſſolved a large portion of ſalt. If anxiety was great, or nauſea and vomiting troubleſome, I have alſo obſerved benefit from the application of a bliſter to the epigaſtric region. Opiates, joined with remedies which had a tendency to determine to the ſurface, were found to be ſerviceable; and wine, with a ſupply of freſh and cool air, in moſt caſes, was highly neceſſary. This method of proceeding will, perhaps, be thought unwarrantable; but I can ſpeak confidently of its ſafety; and I may farther add, that unleſs ſome decided ſteps are taken to change the nature of the diſeaſe, during the continuance of this ſtage, our future endeavours to do good, will generally be in vain. I have hitherto promiſed ſucceſs in the cure of this fever, with a good deal of confidence; but if it ſhould ſo happen, that we are not called to the patient till the yellowneſs has ſpread over the whole of the body, or till the black vomiting has begun to make its appearance, the proſpect, I muſt confeſs, is then very dark. The ordinary reſources of our art are feeble; and if good can be done at all, it can only be done by means, which in the common opinion of practitioners, border on raſhneſs. In this latter ſtage of the complaint, ſo great a degree of torpor overwhelms the powers of life, that remedies do not produce their uſual effect, and our labour is often the ſame, as if we attempted to reſuſcitate a corpſe. I have, however, ſeen inſtances of ſuch unexpected recoveries from the moſt hopeleſs ſtate in fevers, that I ſeldom totally deſpair as long as life remains. I know that

death may be prevented, even after black vomiting has appeared with all its terrors, if a remedy can be found powerful enough to excite the action of the extreme veſſels, and to recall the determination to the ſurface of the body. For this purpoſe, I have employed alternately warm and cold bathing with ſucceſs. I have even wrapt the body, as I mentioned before, in a blanket, ſoaked in water, in which a large portion of ſalt was diſſolved, or which had been ſteeped in brandy or rum, enjoining at the ſame time the liberal uſe of wine, or even more powerful cordials. I have heard of ſome well-atteſted inſtances, where plentiful draughts of rum and water, have checked the vomiting, and apparently ſaved the lives of patients, after the medical people had given them up for loſt.

I have now mentioned the method of cure which I purſued in the yellow fever of Jamaica; and I muſt be allowed to add, that the general indication appears to be confirmed by a view of the hiſtory and progreſs of the diſeaſe, as alſo by conſidering the appearances which are found after death. It was obſerved in the preceding pages, that the circulation became languid at a certain period in the courſe of this fever, and that the determination was, in fact, turned upon the internal parts, particularly upon the alimentary canal, and biliary ſyſtem. To ſupport, therefore, or to recall the determination to the ſurface, where it had begun to retire, was the principal aim which was kept in view. It was purſued with vigour; and, I have the ſatisfaction to add, frequently with ſucceſs. I am afraid that the means may be thought hazardous; but I have never yet perceived from them, even a momentary harm. I ſhall not therefore ceaſe to recommend them, till I find that others have tried them fairly, and found them dangerous or ineffectual.

SECTION II.

IN the preceding pages, I attempted to deſcribe the diſeaſe, which has been uſually regarded as the proper yellow fever of the Weſt Indies, detailing at the ſame time, the particular ſteps of a method of cure, which I have cauſe to believe, was followed with more than ordinary ſucceſs. I now proceed to conſider another ſpecies of diſorder, which frequently makes its appearance among people newly arrived in hot countries, and which, from ſome ſtriking marks of affinity, I have been induced to rank as a ſpecies, or variety of the former. Yellowneſs, indeed, is not by any means common to it, and black vomiting is actually rare; yet paroxyſms and remiſſions are ſcarcely diſtinguiſhable, and the difference between it and the preceding, is perhaps, in fact, only accidental.

I remarked before, that this ſpecies of diſeaſe, as well as the yellow fever, properly ſo called, appears but rarely among thoſe who have reſided any length of time in tropical climates. It was obſerved to begin, as fevers uſually do, with diſagreeable affection of the ſtomach, with languor, debility, and pain of the head. The horror of ſhivering, ſo common in the commencement of febrile diſeaſes, was ſeldom great in degree; but it often laſted long, and ſometimes was accompanied with unuſual feelings. The pulſe was generally ſmall, frequent, and eaſily compreſſed; the eyes were watery, muddy, or inflamed; the features were confuſed, and the countenance was ſometimes flushed: the thirſt was ſeldom great; and the heat of the ſkin was uſually moderate; but a deep and heavy ſighing, a hurried reſpiration, with an inconceivable diſtreſs and anxiety about the præcordia, gave ſtrong indications of the nature of the complaint.—In ſome inſtances I have known ſuch

fevere and excruciating ſpaſms, as, in ſome meaſure ſeemed, to ſuſpend the ordinary functions of life.

In twelve hours, or leſs, there was often ſome abatement in the violence of thoſe ſymptoms; but ſeldom ſuch material relief, as, with any degree of juſtice, could be called a remiſſion. The ſkin became cool, and ſometimes moiſt; yet there ſcarcely ever was any ſweat. The pulſe became fuller, and often leſs frequent; the reſtleſſneſs and anxiety were ſometimes ſenſibly diminiſhed; and the local pain often abated; but this reſpite was neither long, nor of certain duration. In a few hours, all the ſymptoms returned with aggravation. The eyes became more muddy; the countenance more confuſed; the head-ach, and other pains increaſed, together with ſenſations of anxiety, and reſtleſſneſs, hurried reſpiration, and deep and heavy ſighing. The pulſe was now more frequent, ſmaller and harder; the thirſt was increaſed, with naufea, and ſometimes with vomiting. The vomiting was ſeldom bilious: it was not often, indeed, that the matter thrown up, was altered from what had been drank, unleſs by having acquired an additional degree of ropineſs.—To the above ſymptoms was ſometimes added an obſtinate coſtiveneſs, ſometimes ſuch a degree of purging and griping, as might eaſily be miſtaken for proper dyſentery.

As the diſeaſe advanced in its progreſs, the abatement of the violence of ſymptoms, which at firſt was ſometimes perceived towards the mornings, became gradually leſs and leſs perceptible, and at laſt was ſcarcely to be diſtinguiſhed. The anxiety and reſtleſſneſs were now particularly diſtreſſing; the ſkin was ſometimes dry, though oftener moiſt, and in point of heat below the ordinary temperature of health; while it gave the idea to the perſon who felt it, as if there was a powerful ſpaſm ſubſiſting on the ſurface. I may likewiſe remark in this place, that a beautiful red colour of the cheeks, together with a ſmooth-

neſs and cherry plumpneſs of the lips, was frequently obſerved towards the latter periods of the diſeaſe. Yellowneſs, as was mentioned before, was ſeldom ſeen, unleſs in the very laſt ſtage of the illneſs; and vomiting of black, or even bilious matter was extremely rare. There was, however, at all times, a great diſpoſition to faint, with more or leſs of a certain ſpecies of low delirium.

The courſe of this ſpecies of the diſeaſe, was leſs rapid, than the courſe of that which terminates in black vomiting; the termination of the one being often protracted to the eighth or ninth day, that of the other ſeldom exceeding the fourth or fifth. The marks of criſis, as was obſerved before, were rarely diſcoverable in the firſt ſpecies of the yellow fever. They were likewiſe obſcure in the preſent, and I cannot pretend to ſpeak with confidence, of the influence of critical days. Where the termination was favourable, the pulſe became gradually ſtronger, and leſs confined in its ſtroke; the ſkin likewiſe became ſofter, while the impreſſion, which it made on the hand that felt it, communicated an idea that the circulation was more vigorous, and the ſpaſm on the ſurface leſs obſtinate; the eye and countenance likewiſe brightened up; the anxiety and reſtleſſneſs vaniſhed or decreaſed, and ſome appetite for food returned; but it was often difficult to mark the point of time preciſely, at which this change took place.—It may be obſerved likewiſe, where the termination was fatal, that death approached in two different ways. A patient, apparently poſſeſſed of vigour, was ſometimes ſuddenly ſeized with coma or convulſions, and died unexpectedly; but it happened more frequently, that the powers of life were gradually and ſlowly extinguiſhed; the pulſe became weaker and more confined in its ſtroke; while the natural heat and circulation retired by degrees from the ſurface and extremities of the body.

The cure of this ſpecies of the diſeaſe, though by no means eaſy, was leſs difficult upon the whole than that of the former. Inſtead of the torpor and inſenſibility, which prevailed in the latter periods of the proper yellow fever, the mobility of the nervous ſyſtem was ſo much increaſed in the preſent ſpecies of diſeaſe, that remedies ſeldom failed of producing ſenſible effects: and wherever remedies produce effects, it generally is in our power to manage the buſineſs in ſuch manner, that ſome good may ariſe. It may be obſerved in the firſt place, with regard to the cure, that bleeding, which frequently was uſeful in the former ſpecies, was generally hurtful in the preſent; and that inſtead of retarding, it oftener accelerated the progreſs of the diſeaſe. Emetics were employed very commonly by the practitioners of the Weſt Indies, in this as in other caſes of fever; but I cannot help remarking, that languor and debility, frequently yellowneſs, and ſometimes a continual vomiting, which no remedies could reſtrain, were often the conſequence of antimonial emetics of ſevere operation; and I have no doubt in ſaying, that the approach of death was actually haſtened, in ſeveral inſtances, by this method of treatment. Laxatives were occaſionally of ſervice; but the ſtronger purgatives were frequently hurtful. Bliſters were often extremely beneficial; but it requires care and diſcernment to apply them in the proper circumſtances, ſo as to reap the full advantage. Opiates were ſometimes ſerviceable, and bark and wine, in moſt inſtances, were remedies of great value; but the principal truſt was placed in warm and cold bathing; which, under proper management, ſeldom failed of anſwering every expectation completely, or ſpeedily, of removing the chief ſymptoms of danger. Sometimes it appeared to cut ſhort the courſe of the diſeaſe abruptly.

SECTION III.

I HAVE now described two species of a fever, which seems to be, in some degree, peculiar to the natives of northern regions, soon after their arrival in the West-Indies. In the one, a determination to the alimentary canal and biliary organs, with marks of putrescent tendency in the general mass of fluids, was discoverable at an early period; in the other, the brain and nervous system were more particularly and principally affected; while the species, of which I now attempt to give some account, exhibited strong marks of vascular excitement, with a very high degree of the apparent inflammatory diathesis. This was more irregular in its appearances and more complicated in its nature, than the others. The marks of inflammatory diathesis were generally very apparent in the beginning; but they usually gave way or became complicated in the latter stages, with symptoms of putrescency or nervous affection. In describing the history of this disease, it may not be superfluous to remark, that there is seldom any thing particular in the sensations of debility and horror, which precede the formation of the paroxysm. The hot fit was generally observed to run high; the heat was often intense; the pulse, which was quick, frequent and irregular, vibrated often in an uncommon manner, and with an usual degree of force; the thirst was sometimes immoderate, sometimes not greatly increased; the countenance was flushed; the eye glistened, and appeared frequently to be in some degree inflamed; the signs of excitement were in general uncommonly high; yet the disposition to faint was sometimes sudden and unexpected. It deserves farther to be remarked, that blood drawn from the arm did not commonly exhibit the usual buffy appearance of real inflammatory

diatheſis; and though times of aggravation and alleviation were often diſcernible; yet they did not happen at regular and ſtated periods.

It was obſerved frequently, that many of the leading circumſtances ſuffered a material change, about the third day of the diſeaſe. The ſymptoms of high inflammatory diatheſis, which prevailed in the beginning, became mixed, more or leſs, with ſymptoms of putreſcency, or nervous affection. Delirium made its appearance; ſometimes it ran high, with ſtartings and ſymptoms of violent excitement; ſometimes there was a low and muttering incoherence with marks of languor and debility. The gums turned red and ſpungy, and ſometimes bled; the thirſt was frequently intenſe, the tongue dry, with vomiting and ſevere retching; yet vomiting of bilious or vitiated matters was a rare occurrence. The above ſymptoms generally went on to increaſe, during the ſpace of ſix or ſeven days, about which period the powers of life either yielded to the diſeaſe, or ſigns of recovery began to appear: the marks of criſis, however, were ſeldom diſtinct and final; neither was the influence of critical days ſo much to be depended upon as in the common remitting fever of the country.

It was mentioned above, that the nature of this fever was more complicated than that of the two former; ſo the indications of cure are likewiſe more difficult and perplexed. If we proceed on the firſt obvious view of the diſeaſe, we ſhall often do irreparable miſchief by copious and repeated evacuations; yet there will not be leſs danger, on the other hand, if, regardleſs of the preſent degree of excitement, we indulge freely in the uſe of ſtimulants. It is neceſſary to obſerve a middle courſe; and I muſt confeſs, that it is ſometimes difficult to do any thing, without doing harm. Bleeding was frequently employed in the cure of this diſeaſe, and in moſt caſes, it was a uſeful remedy, though leſs perhaps from its own

effects merely, than from paving the way to other more powerful applications. It is, however, capable of being eaſily carried to exceſs; and ought not to be truſted to wholly for the removing of the irritability, and high degree of excitement, which prevails ſo generally in the beginning of this diſeaſe. After bleeding, emetics and cathartics are employed very freely. I have always profeſſed myſelf an enemy to the practice of giving emetics in the fevers of Jamaica; yet, I muſt confeſs, that antimonials were not only ſafer, but of more particular ſervice in this, than in any other ſpecies of fever, where I have ſeen them tried. Among the great variety of forms which have been recommended by practitioners, for the purpoſe of emptying the firſt paſſages, I have not found any one anſwer ſo well, as a thin ſolution of the ſal-catharticum, given at different intervals, with a ſmall portion of emetic tartar, and ſometimes with the addition of laudanum. The operation of this remedy was extenſive. It might be ſo managed, as to promote naúſea or vomiting, ſweat, or moderate evacuations downwards; at the ſame time that it proved very powerfully ſedative. I may likewiſe add, that I have ſometimes found benefit from nitre, camphire and opium, given in pretty large doſes, and accompanied with plentiful dilution. But though theſe remedies were often ſerviceable, and contributed in many caſes to moderate the high degree of irritability; yet the chief dependence of the cure was much better truſted to cold bathing. After the ſurface of the body had been ſufficiently relaxed, by the previous uſe of warm bathing and fomentations, the effects of cold bathing were wonderful. The exceſſive irritability was moderated or removed, and the powers of life were invigorated in a very ſingular manner in conſequence of it.

I have attempted in the preceding pages, to give a ſhort view of a diſeaſe, which has not, I believe,

been hitherto very accurately described by authors, or treated with much success in practice. It is a disease of a continued kind; and, as I said before, in some degree peculiar to the natives of northern latitudes, soon after their arrival in the tropical climates. I cannot help thinking, that it may be easily distinguished, even in the first hours of its attack, from the intermitting or remitting fever, which is the common endemic of hot countries: but I must at the same time add, that this distinction does not reside in the presence or absence of one individual symptom. The state of the pulse, indeed, conveys information, that the disease is not of the kind which has paroxysms and remissions: yet this information can only be obtained from a knowledge and actual comparison of the two diseases:—I do not pretend to describe it in words. The state of the eye and countenance, was likewise observed to be strongly descriptive of the nature of the disease; as also were the deep and heavy sighing, the hurried respiration, the anxiety and restlessness, with a certain uncomfortableness of sensation, which no words can express; but I confess myself, at the same time, perfectly at a loss to fix on any one single symptom, which appearing at an early period, discriminated it with certainty from all other fevers. I have described it under three distinct and separate forms; but I must also add, that they may sometimes be found to be more complicated with each other, than they appear to be in the above description.

CHAP. XII.

INTERMITTING FEVER OF AMERICA.

HAVING endeavoured in the preceding treatise, to give a more accurate history of the endemic fever of Jamaica, than is met with in books, and I am disposed to flatter myself, having pointed out a more successful method of cure than that which has been generally pursued; I shall now add a few observations on the intermitting fever of America; a disease, in which my experience has been tolerably extensive. The frequent occurrence of intermitting fevers in every climate, together with the full and ample manner in which the disease has been treated of by many learned and ingenious writers, excuses me from entering into a minute and full discussion of the subject. I shall therefore employ only a few pages in attempting to illustrate particulars in the history of the disease, which have been superficially noticed; or to explain some points of treatment, which, though not new, I have ventured to carry farther than is usual in common practice. As I had the opportunity of attending to the history of the intermitting fever in several of the southern provinces of the continent of North America, I shall first mention the more constant and general course of the disease, and afterwards point out those circumstances of peculiarity, which seemed to arise from the difference of climate, or from the influence of the season of the year. I shall likewise occasionally take notice of the general state of health of the troops who were employed on the same expedition,

though I muſt alſo add, that I can only pretend to trace the progreſs of the fever with accuracy, in the regiment in which I had the honour to ſerve.

I ſhall attempt, in the firſt place, to give an accurate deſcription of the paroxyſm of an intermitting fever, marking as carefully as I can, the order of ſucceſſion, in which the ſymptoms moſt uſually appear. We are taught by the deſcriptions of moſt writers, to conſider languor and debility as the firſt feeling or firſt eſſential ſymptom in the paroxyſm of an intermitting fever; but I cannot avoid remarking, that an unuſual affection at ſtomach, a flatulence,—in ſhort, ſomething diſagreeable, which I cannot eaſily define, but which was accompanied in many caſes with head-ach, and ſometimes with drowzineſs, preceded every ſenſation of languor or debility in moſt caſes, where my obſervations were made with ſuch care that they could be truſted to. I may alſo farther obſerve, that, as ſoon as this languor or debility began to be perceived, the veins began to ſubſide, the nails turned pale, and at laſt blue; the ſkin of courſe was dry and conſtricted; and there was ſometimes an evident diminution of heat, particularly of the heat of the extremities. To theſe ſymptoms was often added, a diſagreeable kind of yawning, with ſtrong ſenſations of wearineſs, and an irreſiſtible inclination to ſtretch the limbs. A ſenſation of cold was now felt in the back, as if water ran down upon it in ſeparate ſtreams. It ſoon vaniſhed, indeed; but ſuddenly returned again in a more violent degree; in which manner it went on, ceaſing for an inſtant, and then recurring with aggravated violence, till the whole body became at laſt affected with rigour or ſhaking, accompanied, in a more eſpecial manner, with chattering of the teeth. The coldneſs having now arrived at its acme, or higheſt point of intenſity, glowings of heat were perceived in the intervals between the rigors or ſucceſſions. Theſe glowings grew gradually ſtronger, and continuing for a greater

length of time, at leaſt baniſhed every ſenſation of cold. The heat, which now ſucceeded, was often much above the temperature of health; marks of fever ſometimes ran high; the veins became full; the face was flushed, and the ſurface of the body bore marks of diſtenſion. The duration of this ſtate was uncertain: ſometimes it did not continue the ſpace of one hour, ſometimes it laſted four or five. A dampneſs at firſt began to appear on the forehead and breaſt, which extending itſelf gradually to the extremities, was at leaſt formed into a ſweat; in conſequence of which, the fever gradually ſubſided, and the body returned nearly to its natural ſtate.

The above are the moſt uſual ſymptoms of the paroxyſms of an intermitting fever. I have deſcribed them in the order of time in which they moſt uſually appear. I muſt however remark, that ſymptoms are ſometimes obſerved different from thoſe which I have now taken notice of; as alſo, that the order of ſucceſſion, which I have mentioned, is not, by any means fixed and invariable. It is impoſſible to deny the common obſervation, that languor or debility is a general and early ſymptom in almoſt every ſpecies of fever; but it is likewiſe certain, that there are many inſtances, where it is not in our power to perceive its actual preſence. It is therefore precipitate to conclude with Dr. Cullen, that all the future phænomena depend upon this, as their eſſential and original cauſe. There may frequently be deception in attempts to deſcribe the ſituation of others; but that which we feel ourſelves is more to be truſted to:—and I can affirm, that I have often felt ſenſations of cold in my own perſon, previous to every feeling of languor or debility; previous, I might even ſometimes ſay, to any perceptible deviation from a ſtate of health. But beſides, that the exiſtence or perception of languor and debility, does not ſeem to be eſſential to the exiſtence of a paroxyſm of intermitting

fever, I may likewiſe add, that I have ſeen inſtances, particularly in the hot months of ſummer, where the whole of this diſeaſe paſſed over, without the leaſt perceptible degree of a previous cold fit. It happened ſometimes alſo, that during the continuance of the paroxyſm, there was ſcarcely any obſervable diſorder in the pulſe, or any material ſigns of external fever. The tumult and uneaſineſs, which terminate in moſt caſes by ſweat, went off in ſome by urine or ſtool, or perhaps declined in others, without the appearance of any preternatural evacuation. In like manner it was commonly obſerved, in the diſeaſe diſtinguiſhed by the name of partial intermittent, that there was not any perception of cold, nor increaſe of heat; no diſorder in the pulſe, or preternatural evacuation; in ſhort, not a ſymptom, which characterizes the genius of the diſeaſe, except local pain, which continuing for a certain time, diſappears, and then returns again at a ſtated hour. To this we may add, that there are various inſtances, where the whole duration of a complaint, which indiſputably depends on the cauſe of intermitting fever, is occupied by a comatoſe diſpoſition, by convulſions, or even by tetanic affection. If we therefore conſider theſe phænomena attentively, we ſhall find little cauſe to believe, that the moſt uſual ſymptoms of the intermitting fever, are ſymptoms without which the diſeaſe cannot exiſt; or that they are mutually the cauſe and effect of each other. The order of ſucceſſion I have obſerved is not fixed invariably; and caſes are numerous, where thoſe ſymptoms, which ſome authors have conſidered as abſolutely eſſential, do not appear at all. This fact is certain; and we may ſafely conclude from it, that the main hinge of action in a paroxyſm of fever has not been yet diſcovered.

The vital and natural functions are variouſly affected, not only in different people, but in the ſame perſon, in the different ſtages of a paroxyſm of the

ſame fever. The pulſe, in the firſt approach, is often remarked to be ſlower than natural, ſometimes it is more languid and weak. It ſoon however becomes more frequent, though it continues for the moſt part ſmall and contracted, till the latter ſtage of the cold fit. It then uſually acquires ſtrength and ſome degree of fulneſs, ſometimes greater frequency and hardineſs; but as the ſweat begins to flow, the hardneſs and frequency abate, while the fulneſs increaſes; ſo that it returns by degrees nearly to its natural ſtate. The diſagreeable affection of ſtomach, (which I formerly obſerved was ſometimes the firſt perceptible ſymptom of a paroxyſm of the intermitting fever) increaſes frequently to nauſea or retching, ſometimes to ſevere and continual vomiting; which does not ceaſe till ſweating has become general all over the body. The reſpiration, which in the beginning of the paroxyſm, is uſually ſlow, and ſometimes interrupted with ſighing, in the progreſs of the hot fit becomes frequent, laborious and high. It often happens, likewiſe, that there is more than ordinary dulneſs of perception in the mental faculties in the firſt approach of the fever; while this is often ſucceeded by extraordinary acuteneſs in the more advanced ſtages, particularly during the continuance of the hot fit. But though it is only during this period that excitement and delirium are obſerved to be common; yet inſtances are not wanting, where derangement of intellect is among the firſt ſymptoms of the diſeaſe, and where it continues among the principal throughout the whole of the courſe. To the above appearances we may add, that the urine, which is thin and pale in the firſt ſtage, becomes high coloured in the progreſs of the hot fit; and as the ſweating advances, thick and turbid, frequently with the addition of a copious lateritious ſediment.

I remarked formerly, in treating of the remitting fever of Jamaica, that certain hours of attack were

in a very peculiar manner connected with the different types or forms of that diſeaſe; but I cannot pretend to ſay, that the ſame rules were obſerved to hold good, with any degree of certainty in the intermitting fever of America. Single tertians, indeed, began moſt uſually about twelve; though there were likewiſe many inſtances where they came on ſo early as ten in the morning, or ſo late as two in the afternoon. The other forms were ſtill leſs regular. It was alſo taken notice of, that anticipations were common in the ſingle tertian of Jamaica; as alſo that they were irregular and long. In America they were ſtill more frequent; but ſeldom exceeded one or two hours at once. They often, however, prevailed to a certain acme, or point in the diſeaſe, obſerving a regular interval of time in their progreſs. It ſometimes likewiſe happened, that the type poſtponed gradually, till the complaint diſappeared finally, This, however, was much more rare than the other.

Having mentioned, in the preceding pages, ſome general reſemblances of the intermitting fever of America, I ſhall next trace its peculiarities in the different provinces, in which the regiment to which I belonged, had the fortune to ſerve. I may obſerve, in the firſt place, that I joined the firſt battalion of the 71ſt regiment, on York Iſland, in the beginning of the ſummer 1778. Few of the men were then ſick; neither did the number increaſe materially, till towards the latter end of June. The intermittents, which appeared previous to this period, were generally ſingle tertian; and of perfectly eaſy treatment. In the month of July, a dyſentery, of a very particular kind, became epidemic, and the ſporadic intermittent inſtantly vaniſhed. The ſtools in this complaint were numerous and bloody, the gripings were ſevere, but there was ſeldom any very material diſorder in the pulſe. The diſeaſe did not often terminate in leſs than ſeven days; ſometimes it continued a

fortnight or longer. The ordinary treatment was very rarely of benefit; yet the complaint was of a nature ſo little dangerous, that I do not recollect a ſingle perſon who died of it. It diſappeared totally about the beginning of Auguſt, or rather changed into an epidemic intermittent, the type of which was uſually ſingle tertian. The paroxyſms of this fever were regular, the intermiſſions were diſtinct; and its nature was ſo far from being obſtinate, that I ſcarcely met with an inſtance which reſiſted the Peruvian bark, where that remedy was given in ſufficient quantity. This fever continued highly epidemic during the months of Auguſt and September. The frequency of new attacks was conſiderably diminiſhed in the month of October; yet ſuch as happened then, were generally accompanied with dangerous and alarming ſymptoms. Relapſes were common. But though the intermitting fever of this iſland was epidemic in a conſiderable degree, it was not by any means of a fatal nature. If neglected in the beginning, foundation was ſometimes laid for obſtinate complaints; but the diſeaſe was not fatal in its proper form to any one patient, who remained with the regiment. I cannot ſpeak with certainty of the iſſue of a few of the worſt caſes, which were ſent to the General Hoſpital, on the breaking up of the encampment in the month of November. The regiment was then embarked in tranſports, on an expedition to the ſouthward. The ſick were collected into one ſhip, which, after a ſtormy and tedious paſſage, arrived with the reſt of the fleet at Savanna, in Georgia, in the latter end of December. The voyage had an excellent effect on the health of the men. Out of a hundred and twenty convaleſcents, who embarked at New York, in the month of November, not a man died; and there only remained two, who were unfit for the ſervice of the field, on the day of our arrival in the Savanna river. During the months of January,

February and March, the battalion of the regiment in which I ſerved, was a total ſtranger to ſickneſs. It was employed in long and almoſt continual marching, till the latter end of April, when, encamping at Ebenezer, on the Savanna river, the intermitting fever ſoon made its appearance, and ſpread ſo rapidly, that before the end of June, very few remained, not only in this regiment, but even in the garriſon, who had not ſuffered more or leſs from this raging diſeaſe. It was commonly remarked in the hiſtory of this fever, that the type during the month of May, was uſually ſingle tertian, till the fifth or ſixth day; after which, paroxyſms were often obſerved daily, though generally unequal in force and duration: that is, the diſeaſe changed about this period, to a double tertian form. But though this was obſerved to be the caſe, during the greateſt part of May, the type of the fever was uſually double tertian, or quotidian, from its very commencement, in the month of June. The diſeaſe was then of the moſt ardent kind. The paroxyſms were ſeldom uſhered in by a cold fit; and the remiſſions, for the moſt part, were very indiſtinct and imperfect. The heat of the weather was exceſſive, during the greateſt part of the month; and ſtrange and alarming ſymptoms occurred frequently in the courſe of the diſeaſe. In ſome caſes a comatoſe diſpoſition, approaching to apoplexy, or rigid ſpaſms, reſembling a perfect tetanus, occupied the greateſt part of the paroxyſm; in others there were various local pains, deliria, bilious vomitings or purgings, with a multitude of other affections, which appeared on a ſuperficial view to conſtitute the whole of the complaint. Yet theſe ſymptoms declining after ſome continuance, recurred again at a ſtated hour, and were finally removed, or at leaſt ſuſpended, by the Peruvian bark. I left the garriſon of Ebenezer in the beginning of July, and went directly to Savanna, where the ſame epidemic prevailed, though in a de-

gree of leſs frequency, and with ſymptoms of a leſs alarming nature than at the above-mentioned place. At Savanna, it uſually retained marks of diſtinct intermiſſion, and its type was often of the ſingle tertian kind—in ſhort, it was ſimilar to the fever of Ebenezer in the month of May. From Savanna, I went to Beaufort in the beginning of Auguſt. The fever, which uſually prevails at this ſeaſon of the year, in all the ſouthern provinces of North America, was then epidemic among the troops who were ſtationed on this iſland. The type, however, was ſtill more commonly ſingle tertian here, than at Savanna. The beginning of the paroxyſms was likewiſe more generally diſtinguiſhed by a cold fit; and the intermiſſions, for the moſt part, were more perfect and diſtinct. In a few caſes, indeed, marks of malignity were diſcoverable; yet the diſeaſe, upon the whole, was not of a fatal nature, or of obſtinate cure; though unleſs ſpeedily checked by bark, it often degenerated into dyſentery or dropſy, which were not only removed with difficulty, but in the circumſtances under which we laboured, were often of very precarious iſſue. This epidemic was ſtill acquiring force, when the outpoſts were ſummoned to the defence of Savanna. Its progreſs was, in ſome meaſure, ſuſpended during the active ſervice of the ſiege. The enemy, however, had no ſooner retired from before the place, than a fever began to rage with violence, which carried off prodigious numbers, particularly of the foreign troops. It was obſerved in the hiſtory of the preceding year, that few were attacked afreſh with the intermitting fever on York Iſland, ſo late as the months of October and November; but it was likewiſe remarked, that, where the diſeaſe happened at thoſe periods, the ſymptoms were oftener malignant or dangerous. The ſame was in ſome reſpects the caſe at Savanna. The fever, which made its appearance after the ſiege, was of an alarming and violent kind. Marks of diſtinct

intermiſſion were ſeldom diſcoverable, delirium was a common ſymptom, ſpaſmodic affections were ſometimes violent, and the courſe of the diſorder was generally rapid. The rage of this epidemic ceaſed in December; but relapſes continued to return occaſionally, during the following winter; which was an unuſually ſevere one in that ſouthern latitude.

There likewiſe ſtill remained ſome dyſenteric complaints, which reſiſted every mode of treatment that could be deviſed. They yielded however to the return of the warm weather, aſſiſted, in no ſmall degree perhaps, by the active ſervice of the ſiege of Charleſtown. The recovery, indeed, was ſo complete, that, in the beginning of June, the whole of the regiment arrived at Camden in perfect health. The firſt battalion was ſent to occupy a poſt at the Cheraws, on the river Pedee. The diſtance is ſeventy-five miles; yet ſuch was the ſpirit and activity of the men, that they performed the march in three days, without fatigue or inconvenience. An open field, between four and five hundred paces from the bank of the river, was choſen for the encampment of this battalion; while a ſituation perfectly dry and cleared of wood, but nearer to the bank, was reſerved for the encampment of the ſecond, which was not expected to arrive till after ſome time. In a fortnight or three weeks, the intermitting fever began to ſhew itſelf. It ſpread ſo rapidly, particularly in the ſecond battalion, that before the end of July, when the poſt was abandoned, few were left who had not felt its influence. The prevailing ſymptoms of this diſeaſe were much ſimilar to thoſe of the fever of Ebenezer. The type was frequently double tertian, or quotidian; the remiſſions were indiſtinct; the bilious vomitings and purgings were often exceſſive, and marks of malignity appeared in ſeveral inſtances. The approach of the enemy made it neceſſary that the poſt ſhould be withdrawn; but there was much

difficulty in accomplishing it. Two thirds of both officers and men were unable to march; and it was not possible, in the situation in which we were placed, to find waggons sufficient to carry them, together with the necessary provisions and baggage; so that no other resource was left, than to convey some part of them to George Town by water. Boats were therefore collected for this purpose, and such men were put into them, as were judged least likely to be soon fit for the service of the field. These, however, unfortunately fell into the hands of the militia, in their passage down the river, and were soon dispersed into the different parts of the country; so that I cannot speak with certainty of the general issue of the disease. Those who retired to Camden by land, improved unexpectedly in the state of their health, in the course of the march. During the time that we lay at the Cheraws, the remissions were generally obscure; but after the second or third day's march, the type changed frequently from double to single tertian; at the same time that intermissions became clear and distinct. It may be difficult to determine precisely to what cause this might be owing; whether to removal from a situation, where the fomes of the disease was in a very concentrated state; to the mere exercise of travelling; or to the effects of cooler weather with rain, which happened at this time, and which continued for two or three days with little intermission. The whole of those causes, perhaps, contributed to operate this salutary change; though it will probably be reckoned among the first instances, where travelling and getting wet, are recommended as being useful in the cure of fevers. During the month of August, and a great part of September, the army remained encamped near Camden. The weather was excessively hot, and fevers were frequent,—sometimes malignant and dangerous; though they preserved, in general, the distinct character of

intermittents. In the months of October and November relapſes were numerous, and original attacks, though rare, were dangerous and alarming when they happened. Some inſtances of a diſeaſe were now obſerved of a more ſerious nature than any that had hitherto appeared. Inſtead of diſtinct intermiſſions, which prevailed during the preceding months, the ſmalleſt traces of remiſſion were ſcarcely perceptible; the countenance was duſky, and of a greaſy appearance, the tongue was conſtantly dry and parched; the head was often much affected, and grangrenous ſpots ſometimes appeared on the extremities. The duration of this diſeaſe often did not exceed ſeven days; ſometimes it continued a fortnight, or even longer. It was generally of a fatal nature; and where it happened to people who had been ſubject to the intermitting fever in the preceding months, it for the moſt part effected ſuch a change on the conſtitution, as deſtroyed the tendency to relapſe. But beſides this unuſual ſpecies of diſeaſe, which ſometimes appeared in the months of October and November, it was likewiſe obſerved that relapſes of the fever, which preſerved the diſtinct intermitting character, were not only leſs frequent, but commonly leſs alarming, in proportion as the weather turned cooler. Relapſes were often remarked in this ſeaſon to terminate of their own accord, in a very ſhort time; and frequently to leave the body in a ſtate of greater vigour than they found it. I find a fact in my notes, with regard to this ſubject, which is curious and important. Between thirty and forty of the men of the regiment entered upon the ſervice of the campaign in ſo weak a ſtate, that they were unable at firſt to carry their arms. They however gained ſtrength ſpeedily as they proceeded on the march; and ſeldom forgot to mention, that they felt a new acceſſion of vigour after every accidental relapſe. But I muſt further obſerve, that, together with the

above changes which happened in the progreſs of the ſeaſon, the epidemic ſhewed a remarkable tendency to degenerate into dyſentery or dropſy in the months of September and October. The gripings in this ſpecies of dyſentery were often ſevere; the ſtools were large and watery; and times of aggravation and remiſſion were frequently obſerved, as in a regular intermittent. Indeed the intermittent, the dyſentery, and even the dropſical ſwellings ſo often alternated with one another, as evidently ſhewed that they all depended upon the ſame general cauſe. The campaign of the following winter was a very active one. The army travelled over a great extent of country, and was conſidered by many as performing very hard ſervice; but I have the ſatisfaction to add, that notwithſtanding occaſional forced marches, wading of rivers, expoſure to rain, accidental ſcarcity of bread, and no great profuſion of beef, with the total want of rum, the troops enjoyed in general a moſt perfect ſtate of health. Valetudinarians were reſtored to perfect vigour; and when we arrived at Wilmington, in the latter end of April, there ſcarcely was a man in the regiment to which I belonged, who was not fit for the duty of the field. In the ſummer campaign through North Carolina and Virginia, there was no room to complain of hardſhips. The camp abounded with a profuſion of the beſt proviſions; and the marches were ſeldom long or fatiguing. We arrived at Portſmouth towards the end of July, with a very moderate liſt of ſick. Portſmouth is ſaid to be unhealthy; and we ſoon were able to verify the obſervation: an intermitting fever, complicated, or alternating with a dyſenteric complaint, made its appearance ſoon after our arrival, and continued to increaſe during the ſhort time we remained in the place. A diſeaſe of a ſimilar kind continued to prevail in the army, after our removal to York Town. It was not, however, by any means fatal in its nature,

or difficult of cure, if attended to in time, though if allowed to go on, it often degenerated into dropsy, obstructions in the abdominal viscera, or a dysenteric complaint which frequently proved fatal in the beginning of the following winter. The 71st regiment had now served three campaigns in the southern provinces, and might be considered as being perfectly well seasoned to the climate. It was in fact more healthy than any other corps in the army; there not being more than five or six unfit for the duty of the line, when the French and Americans invested the place. After the capitulation the proportion of the sick of the army increased considerably. Some instances of a fever, similar to that which prevailed at Camden and Savanna, in the month of November, were observed in several regiments; but a species of dysentery, which appeared often to have originated from an ill cured intermittent, was the complaint which proved principally fatal.

From the above short history of the intermitting fever, as it appeared in the 71st regiment, in the different provinces of North America, where that corps happened to serve, we may be enabled to form some idea of the changes, which are more constantly produced by season and climate, or which arise accidentally from the particular effects of local situation. In the spring and beginning of summer, the single tertian was the most usual type of the endemic of America, in every province which the regiment visited:—the paroxysms were distinct, and the intermissions were generally perfect. In the months of June, July and August, double tertians were common, and in some situations banished every simpler form. As the weather turned cool, the single tertian resumed its place; so that any other type was scarcely ever seen. But besides the above changes of type, which in some degree followed the changes of season, dysentery or dropsy frequently made their appearance in the months

of Auguſt, September and October, alternating with, or ſucceeding the intermittent; while fevers of a bad and uncommon kind were by no means rare in the months of October and November.

I have thus obſerved in a curſory manner the more general changes of the intermitting fever, as influenced by change of ſeaſon. I may alſo remark, that beſides ſeaſon, climate had a conſiderable effect in modifying the appearances of the diſeaſe. It thus happened, that the type was generally ſingle tertian on York Iſland, even in the heat of ſummer; in ſpring and winter other forms were rarely ſeen. In Georgia, the ſingle tertian was the prevailing form, only in the winter and ſpring. In ſummer, and ſome part of autumn, double tertians were common; and types of ſtill greater complication frequently made their appearance during this period, in ſome particular ſituations of the province. Dyſentery dropſy and dangerous fevers were likewiſe more frequent here in the autumnal months, than they were found to be in the neighbourhood of New York; while the courſe of intermittent, as long as the form was regular, was more ſpeedily checked by Peruvian Bark in Georgia, than in the more northern latitudes. The prevailing type of the climate of South Carolina, was ſingle tertian, even in the ſummer and autumn; yet where the forms of the diſeaſe was in a high ſtate of concentration, as at the Cheraws, the type was often ſo complicated that remiſſions were ſcarcely diſcernible. The tendency of the endemic of this province, to degenerate into dyſentery or dropſy in the autumn, was likewiſe leſs remarkable than in Georgia. The dangerous fevers of October and November were alſo fewer in number; though ſtill more frequent, and more formidable than in the province of New York. The province of Virginia lies about halfway between New York and Savanna; and the general effects of its climate, on the common endemic of the country, cor-

reſponded with its local ſituation.—Deviations from the tertian type were more frequent than at the one place, leſs ſo than at the other.

It appears from what has been ſaid above, that the ſingle tertian is the proper fundamental type of North America. It undergoes, as we have ſeen, a regular change and alteration, in conſequence of the ordinary changes of the ſeaſons, as alſo in conſequence of the effects of the various climates of the different provinces of that extenſive continent: but beſides theſe changes, which are more general and certain, we likewiſe find, that the accidental circumſtances of local ſituation often produce very remarkable effects. In this manner, though the type of the fever which prevailed on York Iſland, was properly ſingle tertian; yet double tertians, and even more complicated forms, were not by any means rare, in a part of the battalion which lay contiguous to a ſwamp. The real nature of the endemic fever of Georgia, is, perhaps, properly of the intermitting kind; yet remiſſions were often ſcarcely perceptible at Ebenezer; which is ſituated immediately on the bank of the river Savanna, and which, in ſome degree, is ſurrounded by creeks of freſh water. It may not be improper to remark with regard to Ebenezer, that few places in America have been obſerved to be more unhealthful; though ſuch a concluſion probably would not be drawn from a general view of its ſituation. It occupies a ſandy eminence of conſiderable elevation, and poſſeſſes a conſiderable environ of cleared ground. At Savanna, which is ſituated twenty-five miles nearer the mouth of the river, there were likewiſe many inſtances of deviation from the ſingle tertian type, but ſtill fewer than at Ebenezer. The fever likewiſe was generally of a leſs dangerous kind. The obvious appearances of the two places did not afford ſufficient reaſon for forming this concluſion. The ſituation of Savanna would have probably been thought

to be the leaſt favourable to health. Though elevated and dry, and poſſeſſing a wider environ of cleared ground than Ebenezer; yet a ſwamp on the right and left, with a river and rice ſwamps in front, threatened great ravage from intermittents. That they were leſs formidable than might have been expected, was probably in a greater meaſure owing to the bluff or ſand bank being higher than the ſituation of the town, and intercepting, in ſome degree, the exhalations from the river and great ſwamps.

I obſerved on a former occaſion, that the ſigns of criſis, in the remitting fever of Jamaica, were generally clear and unequivocal. I muſt now own, that I have not been able to attain certainty, on this head, in the intermitting fever of America. In fevers of a ſingle tertian type, the intermiſſions were frequently ſo perfect and complete, that it was not eaſy to ſay what was wanting to conſtitute perfect health: even in the hot months of ſummer, where the remiſſions were extremely obſcure, I often found it difficult to form an opinion to which I could confidently truſt; as it happened frequently, that thoſe ſigns, which I had been diſpoſed to conſider at one time as marks of final criſis, proved in the event only to be indications of more diſtinct intermiſſion, or of ſome change in the nature of the ſymptoms.

Having given a ſhort view of the hiſtory and progreſs of the intermitting fever of America in the preceding pages, I ſhall now proceed to offer a few obſervations on the manner of treatment. And I may obſerve in the firſt place, that the intermitting fever is not in general a diſeaſe of a dangerous nature. If treated with deciſion in the beginning, it is for the moſt part removed very ſpeedily and very certainly; though if attacked with feeble remedies, it often continues long, and not ſeldom lays the foundation of complaints which eventually have an unfavourable termination. The intermitting fever ſometimes proves

fatal from the actual violence of the ſymptoms of the paroxyſm, though the danger more generally ariſes from a tendency to degenerate into dyſentery or dropſy, or to form viſceral obſtructions. But beſides the danger, which ariſes from the actual force, or from the more tedious effects of the diſeaſe, we often find a character of peculiar malignity, in the intermittents of ſome ſeaſons and ſome ſituations, which deſerves to be particularly attended to. Malignity is a word of a vague meaning; and on different occaſions is differently applied. In the preſent inſtance, I refer the term to a peculiar character of the diſeaſe, expreſſed by a certain ſtate of the eye and countenance of the the patient. It was ſometimes obſerved, that the countenance of the patient was fluſhed; but at the ſame time dark and overcaſt; or that it was of a greaſy and duſky appearance, with a look of ſternneſs and deſpondence in the eye. Thoſe appearances, particularly where a white glutinous covering appeared on the tongue, were conſtantly indications of concealed or lurking danger. They occurred often at Ebenezer and the Cheraws, and I had the misfortune to learn from experience, that whenever they occurred, time ought not to be ſpent in the frivolous preparations, which are uſually thought neceſſary to precede the giving of bark. If we judge it not to be proper in any caſe to venture upon the uſe of bark, before the body has been ſufficiently prepared by emetics and cathartics, we ſhall too often meet with inſtances where the courſe of the diſeaſe will be finiſhed before theſe preparations are completed. It has happened oftener than once to myſelf, that the fatal paroxyſm commenced before I had gone through the uſual routine of preparation, which at that time I thought indiſpenſably neceſſary, before I attempted to cut ſhort the courſe of the fever by means of its well known ſpecific.

Having premiſed ſo much with regard to the ge-

neral nature of the diſeaſe, I ſhall now add a few obſervations about the management of thoſe remedies which have been moſt uſually employed; ſome of which appear to be ſuperfluous; while there is only one of them which has a right to be conſidered as effectual. It is a common and obvious remark, that the intermitting fevers of the ſame ſeaſon ſhew a general tendency to run over a ſimilar courſe, though the modes of treatment may be ſometimes directly oppoſite. As I had often taken notice of this fact during the time that I remained in America, I began to ſuſpect that the changes, which I had been accuſtomed to attribute to treatment, were in reality owing to a diſpoſition in the nature of the diſeaſe, which was little affected by the ordinary preſcriptions. But that I might in ſome degree aſcertain the truth of this ſuſpicion, I ſelected thirty caſes of fever, which had commenced within twenty-four hours of each other. This experiment was made at Ebenezer, in the month of May; where the diſeaſe, though highly epidemic, had not yet diſcovered any ſigns of malignity; ſo that danger was not apprehended from a delay of a few days. I claſſed thoſe thirty caſes in three diviſions, without paying regard to the nature of the ſymptoms of any individual caſe. To one I preſcribed a repetition of emetics, at ſuch intervals as were judged proper; for another, cathartics of various kinds, or managed in various manners; while I left the third wholly to its own courſe. I watched the progreſs of the diſeaſe attentively for the ſpace of eight days, and cannot pretend to ſay, that I obſerved any material difference in the changes or appearance of thoſe caſes, which were treated in ſo different a manner. The type, which was generally ſingle tertian at the commencement of the diſorder, changed for the moſt part to double tertian or quotidian, after the ſecond or third paroxyſm; though not in a different proportion, as far as I could judge, in thoſe which were left

entirely to Nature, or which were treated in the manner which has been mentioned above. It deſerves however to be remarked, that the paroxyſms were uſually milder, more regular and diſtinct after the repeated uſe of carthartics; as alſo that bark ſucceeded more ſpeedily and more certainly after a repetition of antimonial emetics.

Where bleeding is judged to be proper or neceſſary in the cure of the intermitting fever, the circumſtances are generally ſuch as require that it precede every other in order of time. Bleeding was often found to be uſeful in particular caſes. It moderated the violence of ſymptoms, and ſeemed not to be without effect in removing a certain ſtate of the ſyſtem, which reſiſted the ſucceſsful operation of the bark; but I muſt likewiſe add, that there was ſeldom occaſion to employ it in the ſouthern provinces of America. It was blamed by ſome as increaſing the diſpoſition to relapſe: of this, however, I can ſay nothing from my own experience; and as I am diſpoſed to believe, that the loſs of a moderate quantity of blood, ſeldom does material harm in this diſeaſe; ſo I have reaſon to think, that it is ſeldom neceſſary in warm climates, particularly in the hot months of ſummer or autumn.

Emetics have been employed in the cure of intermittents for a long time paſt. There are many practitioners, who conſider there uſe as indiſpenſable; and ſome have pretended, that the cure of the diſeaſe, in its earlier ſtages, may be truſted to this remedy alone. Emetic tartar, ſo managed as to operate at the hour of the fever's return, was ſometimes found to prevent the acceſs of a particular paroxyſm; but though obliged, from want of bark, to have recourſe to this method of treatment in numberleſs inſtances; my experience does not ſupply me with a ſingle one, where I could poſitively ſay, that it abſolutely cut ſhort the courſe of the diſeaſe. Relapſes, I muſt

confeſs, diſappeared frequently in conſequence of the practice; but they likewiſe diſappeared frequently, where nothing at all was done: ſo that I cannot help being of opinion, either that the real effects of emetics have been miſtaken, or that the proper modes of managing them are not generally known. I muſt not however omit to mention, that emetics are occaſionally of great ſervice, and that antimonial emetics eſpecially obviate the effects of inflammatory diatheſis, and on particular occaſions facilitate the ſucceſsful operation of the bark. They are likewiſe ſeldom followed by thoſe dangerous effects which frequently ariſe from the employment of them in fevers of Jamaica; though I certainly ſhould adviſe that they be uſed with great caution in the ſouthern provinces, in the ſummer and autumn, where the remiſſions of the fever are obſcure.

Cathartics have likewiſe been much employed by practitioners in the cure of intermitting fevers; and in few caſes, perhaps, can be omitted with ſafety. They certainly poſſeſs very remarkable effects in rendering the form of the diſeaſe regular and diſtinct; but they do not ſeem to be endued with a particular power of cutting ſhort its courſe.—The circumſtances of the caſe can only point out the propriety or advantage of the various remedies of this claſs.

The above are the uſual modes of evacuation, which moſt practitioners conſider to be indiſputably neceſſary previous to the exhibition of bark. I muſt however remark, that caſes ſometimes occurred in the ſouthern provinces, particularly in the hot months of ſummer, which only admitted of thoſe evacuations in a ſmall degree. Inſtead of the diſtinct and regular paroxyſms, which were expected to follow the uſe of emetics or cathartics, the diſeaſe was ſometimes obſerved to aſſume a languid and continued form, in conſequence of ſuch treatment, while it likewiſe in ſome inſtances recovered the diſtinction of paroxyſm

and remiſſion, by means of applications, which excited the tone and vigour of the ſyſtem; particularly by means of exerciſe in cooler air, or accidentally by expoſure to rain.

Peruvian Bark is the chief remedy, upon which we now depend, for the cure of intermitting fevers. It is a remedy, which like others, has undergone ſome reverſes of reputation, ſince its firſt introduction into Europe; and, though its efficacy is now fully acknowledged, perhaps over-rated by Engliſh practitioners, it does not ſeem yet to have gained the ſame general credit with other European nations. The French uſe it with caution, and many of the Germans are ſtill its enemies. It has been accuſed even by ſome of the Engliſh writers, of failing in the cure of intermittents; and blamed by many of occaſioning complaints more dangerous in their nature than thoſe it was intended to remove. I was early aware of theſe objections, and watched narrowly that I might diſcover its real effects; and am warranted in ſaying, that it has every right to be conſidered as a ſpecific in ague and fever; while it is totally free from the imputation of occaſioning dyſentery, dropſy, or viſceral obſtruction. Thoſe complaints were always moſt frequent, where this remedy had been the moſt ſparingly employed.

But though I have mentioned that bark is both a ſafe and efficacious remedy in the cure of intermitting fevers, I muſt alſo confeſs, that it is only rendered ſo by particular modes of management. It is probable that much of the bark, which is now imported into England, is either in ſome ſhape adulterated, or naturally inferior in quality to what it had been in former times; as we often read of cures effected by a ſingle drachm in the laſt century, which we ſhould ſcarcely now expect from an ounce. Three or four ounces ſeldom failed of checking the progreſs of the moſt formidable fevers of America; one or

two frequently did not produce any ſenſible effect. Being perfectly convinced of the truth of this obſervation, I generally gave bark in caſes, where the circumſtances were judged proper for its exhibition, in doſes of two drachms; which I directed to be repeated every two hours during the abſence of the fever. By this mode of treatment the diſeaſe was often ſo completely conquered, that the patient was frequently capable of returning to his duty in the ſpace of ſeven or eight days. Time ought not to be ſpent in frivolous preparations, or diſeaſes attacked with feeble remedies, where the health of ſoldiers is concerned. The ſpeedieſt cures are generally the beſt; and I have had many opportunities of witneſſing more health and ſtrength gained during eight days in the field, than I ſhould have expected from a month's eaſe in an hoſpital, aſſiſted by the beſt advice of the phyſicians. The above-mentioned quantity of bark, for the moſt part, was ſufficient to effect a cure in ordinary caſes of the diſeaſe; but where danger appeared to be threatening, the doſes were often increaſed to half an ounce, or even more. In ſome caſes of obſtinacy, indeed, accompanied with a ſluggiſhneſs of conſtitution, I obſerved no other rule in the quantity than ſuch as proved diſagreeable to the ſtomach, or excited a tumult in the ſyſtem. The method was often ſucceſsful; and I may obſerve in general, that two ounces taken at five or ſix times, and in the ſpace of eight or ten hours, were often more effectual, than double the quantity in ſmall doſes, and at long intervals. If the large doſes of bark, which I have recommended, ſhould appear to any one to be dangerous or unneceſſary; I may add, that I have myſelf frequently taken an ounce at once; while I have likewiſe obſerved the cure to be tedious and uncertain with the moderate doſes of ordinary practice. I may farther remark, that this remedy was often rejected by the ſtomach, and in ſome caſes

passed off almost instantly by stool; yet that the course of the fever seemed to be no less effectually checked by it, than when such effects did not occur.

The quantity of bark, which I frequently prescribed in the intermitting fever of America, may appear to be greater than necessary: so the time at which it was sometimes given, may also appear to be premature. Bark was seldom given in the fevers of the spring and beginning of summer, unless in cases of relapse, till after sufficient evacuations had been premised. In the autumnal months, where signs of malignity and danger were discovered, the first intermission was often laid hold of: neither was it uniformly deemed necessary, to premise the evacuations of vomiting or purging. In relapses I seldom let pass the opportunity which the first intermission afforded; by which means, those who were subject to the returns of this disease, were rarely returned in the reports of the sick.

Where bark was given in such quantity, and in such manner as I have mentioned above, it generally was successful in checking the progress of the disease; yet I must not omit to observe, that instances sometimes occurred, where it totally failed of this effect, under every mode of management that could be devised. It would be useful to ascertain those circumstances exactly; but this is a task which I cannot promise to perform. It was however remarked, that where the inflammatory diathesis prevailed in very evident degree, bark was frequently given without success: and owing to this cause, perhaps, it was less to be trusted to in the spring, (unless in cases of relapse), than in the hot weather of summer and autumn. But besides the obvious marks of inflammatory diathesis, there appeared to be other conditions of the frame unfriendly to the successful operations of bark. The exact nature of these, indeed, was perceived with difficulty; yet I have often observed

them to be connected with some of the following circumstances; viz. with a small and hard pulse, or with a pulse where the stroke was obscure or without expansion, and where a creeping or vermicular motion was observed in the state of the artery; to which was sometimes added, a constricted state of the skin, a clammy moisture on the surface of the body, without signs of free perspiration, and together with a suspension or irregular action of the nervous influence. In the state described above, bark alone was often given in great quantity, without producing any sensible effect. It sometimes succeeded where antimonials, opiates and other antispasmodics were joined with it; but blisters applied to the back part of the head and neck, were serviceable above all other remedies in removing those circumstances, whatever they were, which stood in the way of its successful operation. In some cases which had proved obstinate to every other means, the disease disappeared immediately after their application; and in every one where they were employed, ceased any longer to resist the bark. Bark alone undoubtedly has a right to be considered as a specific in the cure of the intermitting fever, but its virtues are occasionally improved by the addition of aromatics, chalybeates, and particularly by a certain proportion of snake root. It was a practice with some of the country people of Carolina, to attempt the cure of the intermitting fever by means of Virginia snake root, given in doses of two scruples or half a drachm. I made a trial of that remedy in several instances; but did not find that it was successful. Joined however with the Peruvian bark, in the proportion of two drachms to an ounce, it was often observed to produce very excellent effects. The cures were more complete and more permanent. There was not only less tendency to relapse, but dysenteries and dropsies were more rare, after I was fortunate enough to adopt this mode

of practice, than they had been during the preceding years.

There have been many different conjectures about the mode of the bark's operation in the cure of intermitting fevers; but none of them afford a satisfactory explanation of the subject. Bark is evidently bitter, astringent and aromatic; but how it becomes specific in intermittents, in a degree so superior to all other bitters, astringents and aromatics, is a mystery we cannot easily comprehend. It obviously possesses a power of giving tone and vigour to the powers of life; and often communicates to the sanguiferous system, a certain state or disposition, known by the name of inflammatory diathesis. Thus it is usually observed, that where the nervous frame is weak and delicate, bark rarely failed of cutting short the course of the disease, and its success in such cases, is preceded, for the most part, by apparent changes in the general diathesis of the system. If strength, fulness, and vigour of pulse follow the employment of bark, the fever frequently disappears; but if those signs are wanting, it is not possible to form any certain judgment of the future effect. In like manner, if signs of inflammatory diathesis continue to prevail during the course of the disease, bark sometimes changes it to a continued fever; but seldom checks its progress effectually. From these facts, which I have often seen verified, I am disposed to conclude, that bark is only to be considered as an accidental specific in the cure of intermittents, and that its salutary effects probably may be explained from the change, which it occasions in the relative state of the body. That bark is so often successful depends, we may presume, on the intermitting fever being so often connected with an attonic state of the system; from which cause it probably arises, that it is so much more effectual in summer and autumn, in warm weather and in warm climates, than in the

oppoſite circumſtances. But though an attonic ſtate of the ſyſtem is frequently connected with intermitting fever, it is not conſtantly ſo; hence the diſeaſe is not invariably removed by thoſe proceſſes which excite tone, or give riſe to the inflammatory diatheſis. Exceſſive evacuations and other cauſes, by which the body has been reduced to the laſt ſtate of debility, have often interrupted the courſe of an intermittent. So that we have every reaſon to conclude, that bark is only relatively ſpecific, in as much as it deſtroys certain circumſtances of aptitude, which are eſſentially connected in particular ſituations with the exiſtence of the diſeaſe. In this manner, voyages, journeys, new purſuits, or new modes of life, frights, the active ſervice of the field, or the hardſhips of ſieges have often removed intermitting fever, which had reſiſted the ordinary aids of medicine.

I obſerved in a former part of this treatiſe, that it was ſeldom a matter of much difficulty to ſtop the courſe of the intermitting fever of America; but I muſt now add, that it was always difficult, ſometimes impoſſible to ſecure the patient againſt any future return of the diſeaſe. Bark, though much celebrated for this purpoſe, did not ſeem to prevent a relapſe with the ſame certainty with which it ſtopped the courſe of the fever, when actually preſent. It deſerves however to be remarked, that where bark had been given at regular intervals after the diſeaſe diſappeared, the paroxyſms in the relapſe were, in general, not only ſlighter, but had uſually more of the inflammatory diatheſis joined with them, while the complaint ſhewed more diſpoſition to terminate of its own accord, after a few revolutions. It is univerſally known, that the powers of bark ſeldom fail in the cure of intermitting fevers, where given in ſufficient quantity; yet I muſt alſo obſerve, that its virtues do not ſeem to extend farther than to a temporary ſuſpenſion of the paroxyſms. That bark does not elimenate or

deſtroy the actual cauſe of the diſeaſe, appears plainly from this fact, that relapſes are frequently the conſequence of thoſe circumſtances, which occaſion debility, or which counteract the effects of this tonic remedy. To which we may add, that though relapſes are often of a different type from the original fever, yet, as they generally happen on an even day from the ſuppreſſion of the paroxyſm, there can be little room to doubt that the old complaint again reſumes its courſe, though it probably, in the mean time, loſes ſeveral of its original ſymptoms. It is a fact likewiſe which we ought not to omit mentioning, but which in general, does not ſeem to be much attended to, that ſome periods are more remarkable for the relapſe of intermitting fevers than others. I obſerved before, that relapſes almoſt conſtantly happen on the even days. I now add, that the moſt remarkable of theſe days are the ſixth, the eighth, twelfth, fourteenth, twentieth, twenty-ſecond, twenty-eighth, and thirtieth. The fourteenth is remarkable for relapſes above all the others. Next to it we may rank the twelfth, twentieth, and twenty-ſecond; unleſs in times of very prevailing ſickneſs, where the ſixth and eighth often come in for a great ſhare. If we take pains to examine the particular circumſtances of the patient, and attend to the nature and degree of the prevailing epidemic, we may often be enabled to form a tolerable conjecture with regard to the moſt probable period of return. Having therefore acquired from obſervation ſome general ideas of the different propenſities to relapſe in different ſituations, and in different ſubjects, I uſually began to give the bark in quantity, and to uſe other precautions on the fifth, after the ſuppreſſion of the paroxyſm, in caſes where there were the ſtrongeſt ſuſpicions of a ſpeedy return; while this was delayed till the eleventh, nineteenth, or twenty-ſeventh in others, in proportion to the different degrees of healthineſs. This practice was

continued for the ſpace of three days, or till the ſuſpicious period was paſt. But I muſt further remark, that beſides the propenſity, which was obſerved in fevers to return at the periods above-mentioned, the approach to the new and full moon was likewiſe found to be connected with relapſes in a very remarkable manner. Independent therefore, of the precautions, which were uſed at the ſeptenary periods, the approach to new and full moon was conſtantly attended to.

If the above directions were carefully complied with, we might in general prevent the diſeaſe from proving fatal, or from injuring the conſtitution materially, though I muſt at the ſame time confeſs, that we could in reality do little more than preſerve the patient in a valetudinary ſtate, till cool weather, a change of ſituation, or ſuch a change in the manner of life as excited the active powers of the conſtitution, contributed their part to effect a permanent eſtabliſhment of health. Being perfectly convinced of the truth of this obſervation, I generally remitted the men to the regiment, to be put upon the liſt of duty, as ſoon as I was certain that the courſe of the diſeaſe was actually ſtopt. The practice at firſt ſight may appear harſh; but I have found it to be ſalutary. Exerciſe, even ſome degree of exertion, promotes the recovery of health. Habits of ſloth and indolence are ſpeedily contracted in hoſpitals: the military ardour is gradually extinguiſhed, and bodily ſtrength is recruited more ſlowly than in the field, under every diſadvantage of fatigue or inclement weather.—Of the truth of this I have had ample experience.

I have now pretty fully deſcribed the method of cure, which I purſued in the intermitting fever of America. I am not ignorant that other plans have been adopted, and other remedies employed by others; but as I have not had experience of any, except that which I have mentioned, I do not reckon myſelf

qualified to furnish any remarks on the subject. Bark, indeed, is so safe, and at the same time so effectual, that I should be slow in recommending any other remedy, where this can be procured in sufficient quantity. The strongest proofs of its value arise from a comparative view of the mortality of the intermitting fever, in different regiments, which were employed on the same service, but which were treated in different manners by their respective surgeons.—The Hessians were all of them inveterate enemies to the bark; and there were ever some of the British surgeons who employed it very sparingly. The mortality among the troops trusted to the care of those, was uniformly in great proportion. There was a Hessian regiment, the situation of which I had the opportunity of knowing exactly, that lost one third of its men by this disease and its effects, during one year's service in Georgia. There were British regiments also, which lost more than a fourth; while there were others, which did not lose a twentieth. The whole of these regiments were engaged on the same services; they were all alike foreigners in America; and there appeared to be no obvious cause for so great a difference in the degree of mortality, except a difference in the management of the bark. Bark was scarcely ever employed in one case; in another it was used with timidity; whilst it was given with the earliest opportunity, and in quantities far exceeding the usual practice in the third.

I have described, in the preceeding pages, the method which I adopted in the cure of the intermitting fever of America, whilst that fever preserved its distinct and proper form. I have likewise mentioned the best means I am acquainted with, of guarding against its return; and it will not be improper in the next place, that I add a few remarks on some of its most usual and formidable effects. The intermitting fever of America shewed a strong disposition to

change into a ſpecies of dyſentery, or a purging and griping at particular ſeaſons of the year, and more eſpecially in particular local ſituations. Dropſy was likewiſe a frequent effect of this complaint, and obſtructions of the viſcera were not by any means uncommon, where the intermittent, from neglect or other circumſtances, had been allowed to go on in an uninterrupted courſe.

The changes from intermitting fever to dyſentery, and from dyſentery to intermitting fever, were ſo frequent in the months of Auguſt and September, that thoſe diſeaſes ſeemed evidently to depend on the ſame general courſe; aſſuming at different times the one or the other form from cauſes which we could ſeldom aſcertain. In thoſe caſes of dyſentery the ſtools were uncommonly copious and watery, and remiſſions and exacerbations frequently appeared at regular periods; but though the cure was often attempted by the bark, it did not in general ſucceed.

I muſt begin with acknowledging, that I ſhall not be able to give a complete or accurate hiſtory of the progreſs and final termination of this ſpecies of dyſentery, into which the intermitting fever is ſo much diſpoſed to degenerate; as I ſhall likewiſe only have it in my power to mention the general methods of cure, which were purſued in the earlier ſtages of the diſeaſe. Where it was found that a cure could not be accompliſhed in a reaſonable time in the field or regimental hoſpital, dyſenteric patients were generally removed to places where they could meet with better accommodation; ſo that the diſeaſe in its latter ſtages has ſeldomer fallen under my obſervation.

I muſt obſerve in the firſt place, that this ſpecies of dyſentery had no right to be conſidered as an infectious diſeaſe. It appeared in fact to be no more than an intermitting fever, which, from ſome cauſe or other excited its principal force on the alimentary canal. Remiſſions and exacerbations were generally

obſervable in the one diſeaſe, as well as in the other in the earlier ſtages; yet theſe appearances became gradually leſs and leſs remarkable, and there appeared at laſt marks of permanent affection of the inteſtines. The ſkin now became dry and harſh, the fleſh waſted, and the bowels were uncommonly irritable, particularly where the *prickly heat* had retired from the ſurface; the diſeaſe was now evidently ſupported by the exiſtence of permanent local affection.

With regard to the cure of this ſpecies of complaint I have little to obſerve, which is not generally known. Bark was ſometimes employed to check its courſe; yet I muſt confeſs, that bark alone was ſeldom found to be ſucceſsful. Where there were no marks of an actual inflammatory ſtate of the ſtomach and bowels, it ſucceeded better when joined with aromatics, powder of camomile flowers, and particularly with ſnake root. Laxatives were preſcribed frequently, and ſeemed often to be proper. They were however more uſeful where ſome other thing was joined with them, which had the quality of determining to the ſkin. Opium in various forms was a common remedy; and often a uſeful one before there were marks of permanent affection of the inteſtines. Ipecacuanha, or ſuch preparations of antimony as promoted the evacuations by the ſkin, were frequently combined with it. The great object which I purſued in this diſeaſe was to reſtore and ſupport a free perſpiration, to diminiſh the irritability, and to ſtrengthen the tone of the alimentary canal. I was diſpoſed to expect benefit from warm bathing, frictions, &c. but the ſituation in which we were placed did not admit of a trial of them. Exerciſe was proper, and even ſome degree of exertion. Change of air was ſerviceable in many caſes, well ſeaſoned food in ſome, and wine in others. The above plan was purſued in the beginning of the complaint with tolerable ſucceſs; but if it failed, or could not, from the

circumſtances of the ſervice, be properly executed, the diſeaſe then loſt the remitting form, the body became lean and exhauſted, the ſtools bloody, with a very irritable ſtate of the bowels. In ſuch caſes there was often ulceration, various degrees of inflammation, or obſtructions in the coats of the inteſtines. Bliſters applied to abdomen or loins, and kept open, were often ſerviceable; medicated glyſters, varied according to the nature and ſeat of the affection, were likewiſe of benefit; and in the latter ſtages I have ſeen much good from the employment of ſtrong aſtringents. In illuſtration of this I ſhall mention the caſe of an officer, who was attacked with this ſpecies of dyſentery, ſoon after the ſiege of Savanna. Every thing, which the medical people of the garriſon could ſuggeſt was put into execution, without much benefit. The diſeaſe continued through the whole of the winter without material abatement; the fleſh waſted, the ſkin became dry, with ſuch other ſymptoms as are uſual in this complaint. In the month of March, a perſon of the country recommended the decoction of the bark of a tree, (probably of the ſpecies of the Simarouba,) which appeared to be poſſeſſed of a conſiderable ſhare of aſtringency. It checked the purging and griping inſtantly; ſo that the diſeaſe ceaſed for the ſpace of three weeks. The appetite was good, the ſtools copious, and in ſome degree lienteric. The griping and even purging at laſt returned; the decoction was repeated, but had not the ſame effects. Other aſtringents, the extract of logwood, terra japonica, &c. checked it for a ſhort time, but no material ground was gained. He died in the month of May.

Dropſy ſucceeding, and ſometimes alternating with intermitting fever, was not by any means a rare appearance in America, particularly in ſome ſeaſons, and in ſome ſituations. The ſwellings generally prevailed in every part of the body. They were uſually

leucophlegmatic, though I have alſo ſeen ſome inſtances of tympanitis; a diſeaſe which was commonly ſuppoſed to proceed from improper management of the bark.

With regard to the cure of dropſical complaints, I have little to obſerve which is not generally known. It conſiſted not only in evacuating the water, but in communicating to the ſyſtem ſuch a degree of tone and vigour as reſiſted farther accumulation. With this view exerciſe, and even ſome degree of exertion, was proper: wine, even more ſtimulating liquors, high ſeaſoned food, frictions and warm clothing, were ſerviceable. I ſhould likewiſe have been diſpoſed to expect benefit from ſea bathing; but I cannot venture to ſay, that I have ever made a proper trial of it. Bliſters were alſo uſeful; not only as occaſioning a diſcharge of the waters, but as exciting the action of the vaſcular ſyſtem. Among the numerous claſs of diuretics, there is not any one, which has ſo powerful effects as cantharides, in ſubſtance or in tincture; and among the corroborants, I ſhould be inclined to give the preference to chalybeates, colombo root, and Peruvian bark. It may not be improper to mention in this place, that I have ſeen ſome inſtances where a general anaſarca has been completely cured by the accidental ſupervening of convulſions.

Obſtructions of the abdominal viſcera are likewiſe reckoned among the common effects of intermitting fevers. They are frequently attributed to the early or improper uſe of bark; but appear in fact generally to ariſe from the long continuance of the diſeaſe. I cannot add any thing to the manner of treating them that is not known to every one.

CHAP. XIII.

A GENERAL REVIEW OF THE PRACTICE OF PRECEDING AUTHORS IN FEBRILE DISEASES.

HAVING described the method of cure, which I followed in the remitting fever of Jamaica, the yellow fever and intermitting fever of America; it will not, I hope, be deemed superfluous to give a short sketch of the general principles, which have directed the practice of physicians in febrile diseases, from the days of Hippocrates, till the present times; a subject of which I have not yet seen a connected view. There is reason to believe that the science of medicine made considerable progress in different parts of the world, particularly in Egypt, at an early period; but distinct records of the art, prior to the age of Hippocrates, are either lost, or so blended with the writings which are assigned to that author, that we do not know how to distinguish them. The practice of Hippocrates in fevers, has been accused of being feeble and inert; and it is certain, that many of the most effectual remedies of modern times, were unknown to that venerable physician; yet if we take the pains to estimate candidly the whole mode of proceeding, we shall be forced to acknowledge, that its effects were less inactive than has been generally imagined. The mode of treatment adopted by the Coan Sage, seems not only to have done evident good, but sometimes to have actually cut short the course of the disease. Sweating at an early period, the most effectual means we yet know of cutting off the course of fevers, was employed frequently by this author; and though antimonials were then unknown, yet sweat, excited by a simpler process, was often observed to produce very decisive effects. Hippo-

crates, indeed, has been less circumstantial in the detail of remedies than we could have wished. He has however related the dietetic part very distinctly. His rules are always judicious, and his regimen sometimes of such efficacy, as evidently to operate very considerable changes in the state of the system. It appears frequently to have been his aim to attempt to exterminate the fever in its early periods, or to endeavour to cut short its course abruptly, by direct or indirect opposition to its proximate cause: yet this idea was not pursued beyond a certain point. After the fourth day had passed, Hippocrates usually contented himself with supporting the general powers of life with proper diet and nourishment, allowing nature after that period to perform the work her own way: in other words, to complete the business by the slower operations of coction and crisis. These two views, viz. the attempts to cut short the disease abruptly in its beginning; or in the late periods, the endeavours to support the powers of life, till the natural termination might arrive, comprehend the general rules of practice followed by this celebrated physician: and I much doubt if the moderns, notwithstanding all their pretensions, have actually discovered any other indications more decisive, though I willingly allow, that they have made great improvements in the mode of executing those I have mentioned. At least, I acknowledge for my own part, that I am not yet acquainted with any mode of treatment, by which the natural course of a continued, or even obscurely remitting fever can certainly be prevented, after the first days of the disease are past;—that is, after a distinct formation of the type; before that happened, the sweating process is frequently successful. But though the attempt to cut short the disease in its beginning, or to support the powers of life in the later periods, comprehend this author's general rule of practice; yet we find considerable

diverſity in the manner of accompliſhing theſe different purpoſes. The ἐναντία ἐναντίως, or an attempt to counteract the derangements of morbid cauſes, may be conſidered as the firſt general maxim, which was eſtabliſhed in the cure of diſeaſes. Hippocrates has this maxim conſtantly in his eye, and endeavours by various means, according to a ſuppoſed diverſity in the mode of action, to cut off the immediate exiſtence of fever. In this manner the preſence of heat and bile, or the ſuppoſition of obſtructed perſpiration, have ſeverally furniſhed him with different indications. His ideas however are not preciſe; ſo that his practice frequently fluctuates between conjecture and experiment. If the diſeaſe does not yield to one mode of treatment in a given time, he frequently paſſes to its oppoſite much at random.

The doctrines of Hippocrates acquired ſuch general credit, in every part of the world where they were known, that we do not remark any material innovations in the cure of fevers, till the time of Eraſiſtratus; a ſpace of near two hundred years. The intervening period, indeed, was diſtinguiſhed by philoſophers, who employed their time in inveſtigating the ſtructure and economy of the human frame, as well as by phyſicians, who improved the art of medicine by the invention of new remedies. Plato, Ariſtotle and Theophraſtus were the moſt eminent of the former; Petro is chiefly diſtinguiſhed among the latter. (1) This author, as we find recorded both by Celſus and Galen, attempted to extinguiſh a fever by copious drenching with cold water; ſoon after which, he nouriſhed his patient with wine and ſtrong foods, a cuſtom which was in ſome degree imitated by Clophantus. Hints, however, of the firſt of thoſe practices are found in the writings of Hippocrates, ſo that we may juſtly conſider Eraſiſtratus, as the firſt who departed ſo far from the principles of the Coan Sage, or who arrived at ſo high reputation, as to be

regarded by poſterity as the author of a new method of curing diſeaſes. I mentioned in a former part of this treatiſe, the opinion which Eraſiſtratus entertained concerning the cauſe of fevers. All that we know of his practice may be compriſed in a few words. Eraſiſtratus was every where the inveterate enemy of bleeding. He was likewiſe the author of a certain plan of abſtinence, which, with a little modification from Aſclepiades and Themiſon, made a conſpicuous figure in the annals of phyſic for ſeveral ſucceeding ages.

Herophilus, who lived much about the ſame time with Eraſiſtratus, acquired alſo high reputation in the medical world; but unfortunately we have it not in our power to give a particular detail of his diſcoveries. Soon after the period I have mentioned, the ſcience of medicine was divided into three diſtinct branches; viz. as the art is employed to remove diſeaſes by diet, by drugs, or by manual operation. Serapion likewiſe, who is conſidered as the author of the empiric ſect, made his appearance not long after this diviſion of the profeſſion into ſeparate branches. His followers were numerous, and many of them were reſpectable; but we are not enabled to give a diſtinct account of their practice on the ſubject of fevers. The ſummary views of Celſus and Pliny, or the accidental fragments in the voluminous works of Galen, furniſh only imperfect information. Contentions, however, ran high between dogmatics and empirics, the former of whom were chiefly guided by reaſonings, as the latter truſted ſolely to experience.

The Greeks, for many ages, were the only people we are acquainted with, who cultivated the ſciences with induſtry. For near five hundred years they were almoſt excluſively the profeſſors of the healing art. The Romans were cautious of admitting the refinements of learning into their ſtate; and had

nearly attained the height of their glory, before they willingly received phyſicians into their city. Aſclepiades the Bithynian, the contemporary and friend of Pompey and Cicero, appears to be the firſt, who practiſed medicine at Rome with any degree of reputation. This author left his native country, with the deſign of inſtructing the Roman youth in the arts of eloquence; an acquirement, which was held in high eſtimation among that warlike people; but not ſucceeding in this purſuit, according to his wiſhes, he ſoon diſcovered, that the profeſſion of medicine offered a fair opening for the exerciſe of his talents. The ſtate of the art, indeed, was then peculiarly propitious to his undertaking; the former fame of Hippocrates being divided between Eraſiſtratus and Herophilus, and ſome authors of the empiric ſect. Aſclepiades was two diſcerning not to perceive, that new doctrines could not fail of drawing followers, and too enterprizing not to attempt to carry this purpoſe into execution. He probably, in the firſt place, read over all that had been written by former phyſicians; the moſt eſſential parts of which, he ſelected with a good deal of art, and ſo modified as to form a ſyſtem, which appeared to be complete, and which exhibited upon the whole, conſiderable appearances of novelty. If we take the pains to trace his opinions to their ſources, we ſhall find that Democrites or Epicurus furniſhed him with his philoſophy, that Herodicus ſupplied him with the hints of bathing, friction, and geſtation, that the plan of abſtinence, or faſting for three days, was learnt from Eraſiſtratus; and that Petro and Cleophantus inſtructed him in the practice of giving cold water, and of allowing greater indulgence in the uſe of wine.—But though the authors I have mentioned furniſhed Aſclepiades with the hints of his doctrines and practice; yet we may obſerve, that he has always modelled theſe after his own faſhion, and carried them

farther than had ever been done by their original inventors. It was remarked above, that the profeſſion of medicine was divided into different ſects before the time of Aſclepiades; yet, as far as we can judge from the imperfect information which has been tranſmitted to us, this author was the firſt, who deſerves the name of an active phyſician. Previous to the time of this enterprizing Bythynian, medical men truſted the cure of fevers chiefly to the efforts of nature; and were only ſolicitous about the mode of death. Aſclepiades, aſſuming a bolder principle, endeavoured to cut ſhort the fever in the midſt of its courſe: and it is reaſonable to ſuppoſe, that by ſtepping ſuddenly from the extremes of torture to the higheſt gratification of the appetites, ſuch changes were effected in the ſtate of the ſyſtem, as probably ſometimes accompliſhed the purpoſe. Such were the general views of this author. It may not be improper to add a few remarks on ſome of the remedies which he employed.

It is impoſſible to fix the date, when friction and warm bathing were firſt numbered among the aſſiſtances of the phyſicians. They appear, however, from the teſtimony of Celſus, to have been uſed with caution by the ancients. Aſclepiades not only indulged in them with freedom, but ſo conducted the management of bathing, that it might be juſtly conſidered as a luxury of the higheſt elegance. Geſtation, another of the gymnaſtic remedies employed by this author, even in an early period of ardent fever, is an experiment of a ſtill bolder and more deſperate kind; and ſuch as ſucceeding writers with one voice ſeem to have condemned:—perhaps without examination or fair trial. I am ſenſible that the opinion, which I am to offer on this ſubject, is not likely to meet with general approbation; but the opportunity which I enjoyed, during the late war, of ſerving with a regiment, which was almoſt conſtantly in the

field, enables me to confirm the truth of it by ample experience. The good effects of geſtation or travelling, even in awkward conveyances, were very obvious in almoſt every ſtage and ſituation of the ardent bilious fever; but I ſhall relate ſome inſtances of its ſucceſs, which appear to preclude all poſſibility of doubt. At Ebenezer in Georgia, at a ſeaſon, when the thermometer, in the cooleſt part of the houſe, often ſtood at ninety-ſix, and even ſometimes roſe above it, I was ſeized with the ardent bilious fever, which at that time made dreadful ravage among the troops. For ſix or ſeven days I did not once ſhut my eyes; my thirſt was great, yet every ſort of liquid, which I could procure, was nauſeous; the diſtinction of paroxyſm and remiſſion was no longer perceivable; the pulſe, at the wriſt, was neither uncommonly frequent nor ſtrong; but the pulſation of the deſcending aorta was ſo great, as to ſhake the whole frame; anxiety and reſtleſsneſs were intolerable: in ſhort, the torment was ſo exceſſive, that human nature could ſcarcely ſuffer more. The ſituation was precarious; and without much reflexion I indulged the deſire of being carried to Savanna; though the diſtance was not leſs than twenty-five miles. An open carriage, the only conveyance which the country afforded, was provided for the purpoſe; and I was put into it, in a very feeble and diſtreſſed condition. Fortunately the day was cloudy, and cooler than ordinary. The roads were likewiſe ſoft and ſandy. Though the carriage was very defective, the motion was no ways unpleaſant; and I had not travelled two miles before I felt a wonderful increaſe of vigour. It rained heavily about half way, and before I reached Savanna, I was drenched to the ſkin. The effects which might have been expected, did not follow. Inſtead of being hurt, I was ſurprizingly benefited. I walked into the houſe with ſtrength and firmneſs, eat ſomething without diſlike,

and ſlept ſound the following night; in ſhort, obtained a perfect remiſſion of the fever. This is a ſtrong inſtance of the good effects of travelling in fevers; yet it is only a ſolitary one. I ſhall add another, which places the fact on a ſtill firmer baſis. I mentioned in a former part of this treatiſe, that while the 71ſt regiment lay at the Cheraws, the endemic of the country prevailed among the men in a moſt unuſual degree. The diſeaſe was often without diſtinction of paroxyſm and remiſſion; the anxiety and reſtleſsneſs were intolerable, bilious vomitings and purgings were frequent, and exceſſive. While near two hundred men were in this ſituation, an order arrived for abandoning the poſt. It being impoſſible, as we were ſituated, to provide waggons to tranſport ſo great a number of ſick, about forty of thoſe who were leaſt likely to be ſoon fit for ſervice were ſent down the river in boats. Of the particular fate of this party I cannot ſpeak with certainty; but I have the ſatisfaction to add, that not a man died of thoſe who retired to Camden by land; and that after the third day, ſcarcely a fever was left, which had not aſſumed a regular intermitting form. This appears at firſt ſight almoſt an incontrovertible proof of the ſalutary effects of geſtation; but I muſt not at the ſame time omit to mention, that benefit alſo probably reſulted from a change, which accidentally happened in the ſtate of the weather. The weather, (which, during the time we remained at the Cheraws was uncommonly hot,) became unexpectedly cool after the march was begun; together with rain, from which the ſick men had nothing to ſhelter themſelves. This inſtance of expoſure to rain furniſhes a proof of a fact of much importance. It is generally believed that getting wet with rain is hurtful to a perſon in health. It is no leſs commonly ſuppoſed to be certainly pernicious in ſickneſs; but the contrary appears to be ſometimes the caſe. I have ſeen the happieſt effects from the application of cold, even

from getting accidentally wet with rain in many inſtances, beſides the preſent. Increaſe of tone and vigour was generally the conſequence; and life was evidently protracted, ſometimes perhaps ſaved, by accidents, or modes of treatment, which, in the common opinion of mankind, would have been reckoned the cauſes of death.—But though I have mentioned the accidental good effects of geſtation, and even of expoſure to rain in different ſtates of the ardent fever; I ſhall not be ſo paradoxical, as to recommend ſuch experiments in common practice. I muſt however be allowed to obſerve, that we have little cauſe to be afraid of their pernicious effects. Motion and travelling, as far as my experience goes, were conſtantly hurtful in caſes of local pain and inflammation; or in derangement of intellect; but, on the contrary, conſtantly ſerviceable in anxiety and reſtleſsneſs, depending on the ſtate of the ſtomach; as alſo in affections of the biliary ſyſtem.

I mentioned before, that Aſclepiades practiſed medicine at Rome with great reputation. He propagated his doctrines with a good deal of ſucceſs; but ſuch is, and ever has been the fate of our conjectural art, that no ſyſtem has yet preſerved its credit undiminiſhed for any length of time. The views of phyſicians, with regard to diſeaſes, had hitherto been various and complex; even fevers had been often conſidered, by the ſame perſon, as depending on different cauſes. Themiſon, a pupil of Aſclepiades, attempted to remedy the perplexity which neceſſarily ariſes from this inſtability of arbitrary conjecture; and endeavoured to reduce all the diſorders, to which the human body is liable, to two general claſſes;—viz. to thoſe which ariſe from an increaſed degree of ſtricture, or its oppoſite affection, preternatural laxity; to which he afterwards ſubjoined ſome complaints, which appeared to partake of the nature of both.—The idea of ſimplifying diſeaſes did not probably ariſe

in the mind of Themiſon, till the latter period of his life; on which account, perhaps, the doctrines were left in ſome meaſure unfiniſhed; and it is to Theſſalus, who lived in the time of Nero, that we are indebted for completing the methodic ſyſtem, and for enlarging the bounds of its fame. Soranus likewiſe added to its credit. It is a misfortune, which we muſt regret, that except Cœlius Aurelianus, there is not one of the many authors who were attached to the tenets of this ſect, whoſe works have eſcaped the wreck of accident or time. This only remaining author appears to have copied, by his own confeſſion, almoſt literally from Soranus.

It does not belong to this place to enter deeply into the tenets of the ſect, with regard to diſeaſes in general; but leads to views of ſome importance in the theory and treatment of many diſorders. It proceeds on the ſuppoſition of circulation in all parts of the body; and with a little latitude may be ſuppoſed to comprehend the affections of the animated ſolid or contractile fibre. Fever is conſidered by the writers of this ſyſtem, as a diſeaſe of the claſs of ſtricture; and if we had authority to add ſpaſmodic, we might believe the methodics had diſcovered a very important phenomenon in the hiſtory of febrile diſeaſes.—But this ſome may think is granting them too much. If we take the trouble to trace their doctrine, reſpecting the cauſe of fever, to its ſource, we ſhall find that the hints of it are furniſhed expreſsly by Aſclepiades, who perhaps borrowed his ideas on the ſubject from Eraſiſtratus, or even from Hippocrates.—But as ſtricture, aſſigned by the writers of the methodic ſect as the cauſe of fever, is not perhaps radically different from the obſtruction of preceding authors; ſo we do not find much material difference in their manner of conducting the cure. The followers of Hippocrates, Eraſiſtratus and Themiſon proceeded equally on the idea of reſtoring permeability in the

minuter canals of the ſyſtem; a purpoſe which they conceived would be beſt affected by certain proceſſes of attenuation and relaxation. Thus Hippocrates diluted plentifully, and gave nouriſhment only ſparingly, during the firſt days of a fever. Eraſiſtratus enjoined a general abſtinence, Aſclepiades preſcribed a term for the duration of the abſtinence; while Themiſon limited it ſo rigidly to the ſpace of three days, that the practice was diſtinguiſhed by the name of diatriton, as its followers were known by that of diatritarii. This idea of diatriton conſtituted an object of much importance in the ſyſtem of the methodic phyſicians. It directed all their movements, and is the only view, which can properly be called their own. The mode of application of the remedies of preceding authors was occaſionally modified by this ſect; but except that which I have juſt mentioned, we do not diſcover much that is fundamentally new.

It may not be improper in this place to take notice of the practice of cold bathing in fevers, which was introduced at Rome in the infancy of the methodic ſect; and which afterwards acquired great celebrity in different parts of the world. The Emperor Auguſtus, who for the greateſt part of his life was afflicted with ill health, was at laſt attacked with a complaint of ſo obſtinate a kind, that the uſual applications did not afford him any relief. Warm bathing and all that train of remedies had been tried in vain. The Emperor was ſenſible of his deſperate ſituation; and his phyſician Antonius Muſa, baffled in all his attempts, ventured, though apparently at great hazard, to preſcribe the cold bath. The health of Auguſtus was unexpectedly reſtored by it; and the phyſician was highly honoured, and amply rewarded. It is a misfortune that we do not know the ſpecific nature of the diſeaſe, under which the Emperor laboured; but we have reaſon to conclude, from the circumſtances which attended the cure, that it

was a fever of a bilious remitting kind; perhaps complicated with catarrhal affection, and wasting of the body. But though the success of cold bathing, in the instance I have mentioned, was much greater than expectation; yet the remedy did not long retain its credit. It was soon afterwards employed in the case of Marcellus, a youth of great hopes, and presumptive heir to the empire; but the event proving unfortunate, it fell suddenly into disrepute,—probably without sufficient cause. We find however that Celsus, in less than half a century, afterwards ventured to recommend it in a certain species of the slow or hectic fever; though Charmes, a physician of Marseilles, appears actually to be the first who rendered the use of the remedy general. Galen employed it frequently with great freedom and boldness. The Arabians, particularly in pestilential diseases, went still farther than the Greeks or Romans; and we presume, from the fact recorded by Busbequius, that it was sometimes prescribed at Constantinople, even so late as the sixteenth century. I do not know that it has been often tried in Europe, since the revival of the medical sciences in the West. I mentioned in a former part of this treatise, that I had employed it frequently in the fevers of Jamaica; I now add, that I have ventured upon it in the fevers of this country with so great success, that I should expect the most beneficial effects might result from a proper management of it. (a)

We are indebted to Celsus, who lived in the time of Tiberius, for preserving many of the opinions and practices of preceding physicians, which otherwise would probably have been lost. This author, not less remarkable for candour, than for the elegance and perspicuity of his manner of writing, does not seem to have been blindly devoted to the tenets of any particular sect. He has favoured us with valuable extracts from the works of the most celebrated dog-

matics; he has likewiſe taken notice of the moſt remarkable opinions and practices of the Empirics, without omitting to mention the innovations of Aſclepiades and Themiſon. With regard to his particular merit as a phyſician, we may obſerve that he every where diſcovers an excellent judgment, and that his practice is generally decided without being raſh.

So great have been the ravages of time or accident among the writings of the early phyſicians, that it is ſcarcely poſſible altogether to avoid error in attempting to trace the various revolutions in medical practice, prior to the time of Galen. From that downwards we are enabled to give a more certain and better connected view of the ſubject; there being few of the principal writers of this latter period, who have not been preſerved entire. When Galen came firſt to Rome, which was in the time of the Emperor Antoninus, the practice of medicine was chiefly in the hands of the followers of the methodic ſyſtem. The practice of diatriton, or abſtinence for three days was then in high faſhion, not only with the profeſſed pupils of Theſſalus, but even with the few remaining adherents of Eraſiſtratus. Galen every where declares himſelf its inveterate enemy; and often tranſgreſſes the bounds of liberality and decency, in his attempts to turn it into ridicule. His own endeavours are expreſsly exerted to revive and eſtabliſh the principles of Hippocrates; and to complete thoſe parts, which the want of time or the want of experience of his maſter had left imperfect. He poſſeſſed uncommon fertility of genius, a great flow of language, and a judgment by no means deficient; yet, from a ſophiſtical ſpirit of philoſophizing, he frequently ſo entangled his opinions with theoretical diſtinctions, that his views are often uncertain, and ſometimes embarraſſing. The principle with which he ſets out is directly to oppoſe the actual exiſtence

of fever; he next recommends to remove, at leaſt to avoid an increaſe of thoſe cauſes which give riſe to the diſeaſe. Theſe ideas are drawn from the writings of Hippocrates, and are ſuch as no perſon will diſpute: but, as the cauſes of fever are ſuppoſed, both by Hippocrates and Galen, to be many and various, ſo the indications of cure often require to be executed in different, and ſometimes in directly oppoſite manners. This neceſſarily gives riſe to confuſion; and entangles the practitioner in the mazes of doubt and conjecture; to obviate which, as much as poſſible, the learned commentator of Hippocrates has thought fit to divide fevers into three general kinds, viz. ephemeral, continued, and hectic or habitual; the cauſes of which he ſuppoſes to be ſo little analogous to one another, as to demand particular management in the method of cure.

We look in vain for new views, or material improvements in the management of fevers, in the writings of thoſe Greek phyſicians who followed Galen. Oribaſius profeſſedly is no more than a collector of the opinions and practices of other men; and Aetius, on the preſent ſubject, does not aſpire to much higher fame. There are, indeed, few of his obſervations, which may not be found in the volumes of Galen, or ſome preceding writer; yet he ſeems generally to have comprehended what he wrote. He digeſted the knowledge which he found in books with care and judgment; and gives an arrangement ſo clear and perſpicuous, that the perſon may derive information from Aetius, who would be overwhelmed and loſt in the prolixity of Galen's diſcuſſions.

From thoſe writers, however, who trod implicitly in the footſteps of Galen, we muſt be allowed to ſeparate Alexander of Tralle, a phyſician who lived in the ſixth century. This author wrote his book on fevers at a very advanced age; and though the treatiſe perhaps does not contain many ideas, which may

not, in ſome ſhape or other, be found in the writings of his predeceſſors, yet the obſervations have the appearance every where of having originally ariſen from actual experience. The language, which is conciſe, clear and perſpicuous, is wholly his own. The ambiguous circumſtances of diſeaſes are more accurately diſcriminated than in any preceding work which has deſcended to the preſent times; and though the manner of accounting for things may be ſometimes erroneous, yet it has had little influence on the practical indications, which are almoſt unexceptionably judicious. As Alexander of Tralle wrote at a time of life when fame muſt have been indifferent to him, and to a friend, whom he was more ſolicitous to inſtruct than to amuſe with the ſplendour and variety of his learning, we have an additional cauſe to give our confidence to his obſervations. His manner is candid and ingenious; and the treatiſe before us may be conſidered by the practical phyſician, as the moſt valuable of the remains of the ancients. Judicious cautions are every where interſperſed, and conſiderable changes in the management of remedies are ſometimes attempted; but the practice of giving opiates in a certain ſtate of fever is the only practice of this author, which has any title to be called innovation.

Paulus is the next phyſician of note, who lived after the days of Galen. He was born in the iſland of Aegina, and travelled over many countries. It is probable that he was ſufficiently acquainted with every diſcovery, which had been made by his predeceſſors; yet Galen, on the ſubject of fever, is the author whoſe works he has principally followed. His book on fevers, indeed, contains all the material doctrines and obſervations of that voluminous writer; and thoſe who dread the labour of encountering the prolix and ſophiſtical diſquiſitions of the commentator of Hippocrates, may find a very diſtinct analyſis

of his opinions and practices in the treatise of Paulus Aegineta.

Having endeavoured in the preceding pages to give a short view of the methods which were usually pursued by the most eminent of the Greek physicians, in the cure of fevers, it will be necessary in the next place to take some notice of the improvements of their immediate successors, the Arabians. This task will be soon performed; the Arabians have not in reality opened any views in the curative indications of febrile diseases, which were unknown to their predecessors; or which require that we should spend long time in endeavouring to explain them. The medical science evidently drew its origin from the East; yet it was also soon reconveyed to the countries from whence it sprung, with improvements and additions from the genius of the Greeks. We learn from Herodotus, that Democedes, a native of Crotone, who had studied medicine in the island of Aegina, far excelled all the physicians of the Persian court, even so early as the time of the first Darius; though the court of this Prince probably could boast of all the skill, both of Assyria and of Egypt. Clesias sometime after was held in great estimation by Artaxerxes; and the invitation, which was held out to Hippocrates by the Persian monarch, indicates very clearly, that the Greeks, even then, were more famed for medical skill than the inhabitants of the Eastern countries. The islands and shores of the Mediterranean seem through the whole history of medicine, to have produced the greatest number of physicians. Crotone and Cyrene were famous for several ages: and Alexandria, at a later period, rose into great celebrity. Students flocked to it from every part of the world; it was even necessary that every one, who aspired to wealth or reputation in physic, should spend some time in this celebrated seminary. It was owing perhaps, in some degree, to the vicinity of

this illuſtrious ſchool, that the province of Syria enjoyed at one time, a conſiderable ſhare of learning and learned men. The works of the moſt eminent of the Greek phyſicians were tranſlated into the dialect of the Syrian country, in the ſeventh and eighth centuries; by which means they were probably, in ſome meaſure, propagated in the Eaſt: though we alſo are informed by Abulpharage, an Arabic writer, who had preſerved many curious anecdotes of private hiſtory, that the doctrines of Hippocrates were planted in the Choraſan, at a ſtill earlier age, by the phyſicians, who followed in the train of Aurelian's daughter, who was married to Sapores king of Perſia: nor is it improbable, that theſe doctrines were ſtill more generally diffuſed through the Perſian dominions, by the alliances of friendſhip, as well as by the long wars, which were afterwards carried on between the Greek empire and the celebrated Khorrou Pawiz. But though the inhabitants of Syria and Irak were an enlightened nation, at an early period; their neighbours, the Arabians, who afterwards attained ſo great a name in ſcience no leſs than in war, remained long in a ſtate of illiterate ignorance. Before the eſtabliſhment of iſlamiſm, there ſcarcely was a native Arab, who could either write or read. The little genius they poſſeſſed was chiefly exerted in compoſing verſes, or in colouring a rhetorical harangue. They appear, indeed, to have acquired ſome practical knowledge of the motions of the heavenly bodies; and it is likewiſe reaſonable to ſuppoſe, that they had the ſame ſkill in medicine, as is common to ſavage nations; but there is no reaſon to believe, that they, as yet, had made progreſs in the medical art, conſidered in a ſcientific view. Hareth, a native of Tayef, who lived in the time of the prophet, and who ſeems to have been in habits of intimacy with that ſingular man, is the firſt of the Arabs, whoſe name is recorded among the

phyſicians of the Eaſt. This perſon, who acquired ſome knowledge of medicine at Niſabour, and other places in the Choraſan, returned home after ſome time, with great wealth, and no ſmall ſhare of fame. He practiſed among his countrymen with much reputation; but how far he ſpread the light of ſcience among them is uncertain. The Saracens advanced rapidly in conqueſts and the eſtabliſhment of their faith; but we do not hear any thing of their progreſs in the healing art, till the ninth century. Syrians and Perſians, generally of the Jewiſh or Chriſtian religion, laboured ſometimes for the warlike Arabs in the ſervile occupation of curing diſeaſes, at leaſt we do not know that any of the Saracens attained much eminence in medical ſcience, till the tranſlations of Honain and his pupils laid open to them the treaſures of the Greeks. We are ill qualified at this period to judge of the merit of theſe tranſlations. But if we may be allowed to form concluſions, from the uſe which has been made of them, we ſhall not, perhaps, be diſpoſed to entertain a very high opinion of their accuracy. In many inſtances, the later Arabian phyſicians have expreſſed the ideas of Hippocrates and Galen only very looſely; and in ſome few caſes, perhaps, have not very clearly comprehended their meaning. But, as the later Saracens were ſeldom ſkilled in any language except their own; the original tranſlators are probably alone blameable for the whole of theſe miſtakes.

The medical authors, who wrote in the Arabic language between the ninth and fifteenth centuries, and, who ſtill lie concealed in the leſs acceſſible dreſs of their native country, are almoſt innumerable: neither are thoſe, who have been introduced into the common acquaintance of Europeans few in number, or ſmall in volume. If I poſſeſſed a complete ſeries, even of thoſe who are commonly known in Europe, the examination I have entered upon might be drawn

out to a considerable length; but as I have no hopes of obtaining that soon, I shall content myself with giving some idea of the Arabian system of practice in fevers, from the works of Avicenna, the most eminent and best known of the Oriental physicians. An examination, indeed, of one of the writers of this nation may, in a great measure, render an inquiry into the others unnecessary. Those, at least whom I have seen, do not differ materially from one another; or perhaps essentially from the Greeks who went before them. The canon medicinæ, the principal work of Avicenna, exhibits a systematic view of the whole art of medicine, theoretical as well as practical. I have read over with care all that relates to fevers; and though there is little, perhaps which may not ultimately be traced to Galen or Hippocrates; yet the author has not copied servilely from either of them. He is more full and particular than the one; less prolix and tedious than the other. I must however remark, that the distinctions and divisions, which he has attempted to introduce into the history of fevers, are not only unnecessary, but actually serve to embarrass the indications of cure. His general theories are those of Galen. In the general conduct of the cure, he treads in the footsteps of the same master. He appears, indeed, to be more fearful of the lancet; while he is not perhaps always judicious, or consistent with himself, in the manner of employing it. On the contrary he has admitted cool air rather more freely, and has perhaps carried cold drink even to a bolder length, than had been done by the Greeks. Cool air, cold drink, and even the external application of cold, may be reckoned among the most effectual remedies in the fevers of hot climates; and this author has conducted the management of them, in a luxurious, elegant and efficacious manner. But though the works of Avicenna furnish a general view of the practice of the Arabian school of physic, it is still in some degree a defective one. As he has not

furniſhed us with a detail of the caſe of an individual, we are not able to judge preciſely of his powers of diſcerning the diſeaſe, or of his deciſion in the manner of treating it.

The medical ſcience, which after the taking of Alexandria was little cultivated by the ſlothful Greeks, or barbarous nations of the Weſt, ſprung up with new vigour in the province of Syria, in Irak and Arabia; and followed every where in the train of the Saracen conquerors. Extending with their arms over the northern coaſts of Africa, it ſoon found its way into Spain; and, even ſo early as the eleventh century, was conveyed to Salernum in Italy, by Conſtantinus Africanus, a native of Carthage, who had lived long in Aſia, and who was well acquainted with the language and medical knowledge of the Orientals. The Arabians were the firſt who opened the ſources of chemiſtry; they alſo made great improvements in the art of ſurgery, and even deſcribed ſome complaints which in earlier ages were not taken notice of but they departed but little from the ſyſtem of the Greeks in the management of febrile diſeaſes. After the fall of the Roman empire the genius of learning made no exertion in Europe for a very long period of time. The native European ſlothfully acquieſced in the imperfect knowledge of Arabian writers, which was obtained from the inelegant, and perhaps often unfaithful tranſlations of the Jews, who, for a conſiderable time, were no contemptible profeſſors of the medical art. But, though ſome part of the knowledge of the Arabian phyſicians was communicated, in this manner to the nations of the Weſt, in the eleventh and twelfth centuries; yet a part of the ſixteenth paſſed over, before it was poſſible to trace any marks of improvement. Commentaries were written without number; but, for many years, there ſcarcely was an individual in all the ſeminaries of Europe who dared to think for himſelf. It has been cuſtomary to date

the revival of ſciences in the Weſt from the taking of Conſtantinople, by which the ſtores of Greek literature were in ſome degree opened to the world. The language of Galen began then to be more generally underſtood, and the writings of Avicenna fell rapidly into neglect; yet the advantage which accrued to medicine from the change, does not appear to have been great. The mind was exerciſed in a wider field of learning; but it was ſtill in chains to the authority of the ancients. The opinions of Galen and Hippocrates were copied, recopied and commented upon by hundreds; but there were very few who ventured to uſe any judgment of their own. Among the moſt celebrated of the followers of Galen we may reckon Fernelius, Foreſtus, Lommius and Sennertus, men of conſiderable talents, but who were too ſcrupulouſly devoted to the principles of their maſter, to open a new road in the practice of the art. This was reſerved for Paracelſus, who early in the ſixteenth century ventured to attack the opinions of his predeceſſors, and the authority of Galen. Paracelſus poſſeſſed a conſummate ſhare of aſſurance, together with knowledge of remedies which were not generally known at that time. He acquired ſome acquaintance with the chemical diſcoveries of the Arabians, in the courſe of his various travels, and applied in practice what he had learned, on his return to his native country. He deſpiſed the authority of the regular phyſicians, employed remedies with great boldneſs, and often with ſingular ſucceſs. This ſucceſs was even very probably exaggerated by report; and there appear to have been many, who followed him implicitly; while others exerted themſelves in modifying and improving his ideas. Under this laſt view we may rank Van Helmont, a perſon, who effected a very material innovation in the manner of curing febrile diſeaſes. Van Helmont poſſeſſed conſiderable learning; but diſcovered, at the ſame time ſuch marks of warmth and enthuſiaſm of

genius, as diminiſhed his credit with contemporary and ſucceeding practitioners. The terms which he employs, are ſometimes ridiculous; and his reaſonings are frequently disfigured with fancy and whim; yet his ideas are generally important, and often well founded. The archæus of this author does not differ materially from the ſentient principle (το σπερμωτα) of Hippocrates; and perhaps comprehends the whole idea of the vis medicatrix naturæ of the moderns. Van Helmont proceeds to the cure of fever on the important principle of exciting, or calling forth the powers of life, to exterminate an offending cauſe; ſo that we may actually conſider him as the firſt, after Aſclepiades, who attempted to take the buſineſs wholly out of the hands of nature. He diſregards the proceſſes of coction and criſis; and makes a decided effort to cut the diſeaſe ſhort at an early period. He is likewiſe an enemy to bleeding, purging, vomiting, and the various evacuations which had been employed by his predeceſſors, attempting to accompliſh his purpoſe ſolely by the means of ſweat, and inſenſible perſpiration. The ſucceſs of his practice was ſo great, that he deems the man unworthy the name of phyſician who ſuffers a fever to exceed the fourth day; a degree of ſucceſs, which all the powers of antimony have not yet enabled us to boaſt of.

The circulation of the blood having been proved inconteſtably about the middle of the laſt century, hopes were reaſonably entertained, that the healing art would be benefited by the diſcovery. It does not however appear that medical men, for ſome time at leaſt, either argued more clearly, or practiſed more ſucceſsfully. The advocates of the galenical and chemical ſchools had gradually approached to each other; ſo that the doctrines and practices of thoſe contending parties were now inſenſibly blended together. Sometimes the one mode of thinking predominated, ſometimes the other; but chemical principles every

where gave ſcope to the imagination, which often indulged in the wildeſt extravagance of conjecture. Among the number of thoſe conjecturers, who arrived at much eminence and fame, we may reckon Sylvius de le Bae, who lived in the end of the laſt century, and introduced a conſiderable innovation in the manner of treating fevers. His theories are generally known. They appear to be totally deſtitute of foundation; yet unfortunately are the groundwork of all his practical indications. His principal view conſiſts, in regulating the mixtures of bile and pancreatic juice. He likewiſe lays ſo great a ſtreſs upon the nature of the occaſional cauſe, as gives riſe to doubt and ambiguity. Thus he ſometimes preſcribes acids, though oftener aromatics, volatiles, and opiates. But as we poſſeſs ſome caſes, which he appears to have healed, in the Leyden hoſpital, with all his ſkill and attention, we are enabled with more certainty to form a judgment of the particular merits of his practice. It has not any claim to extraordinary ſucceſs; yet it is evidently innocent of the great harm which ſome later authors have imputed to it. In ſhort, if we except opiates, we may conſider the reſt of his remedies as very feeble and ineffectual.

During the time that Sylvius flouriſhed in Holland, a new theory of fevers was offered to the public in England by Dr. Willis, the celebrated author, to whom we are ſo much indebted for bringing into view the importance of the nervous ſyſtem, in the economy of the human frame. It does not however appear, that this writer's theory ever extended far, or that it was the cauſe of much innovation in practice.

The method of treatment, which was generally adopted in the fevers of England, at the time when Sydenham began to ſtudy medicine, conſiſted principally in bleeeding, in vomiting with antimonials, in evacuating the inteſtinal canal by means of glyſters or gentle laxatives; and, in the latter periods of the

diſeaſe, in attempting to raiſe ſweat by hotter alexipharmics. In the firſt conſtitution of ſeaſons deſcribed by this author, viz. the years 1661,-62,-63,-and 64, we do not find any material deviation from this general plan of cure; which was the plan followed by Willis, and other contemporary phyſicians. In the next conſtitution, viz. the years 1667, 68 and part of 69, Sydenham forces himſelf on our obſervation by an attempt to effect a very important innovation. The fever which prevailed during the laſt mentioned years was generally of long duration. It was uſually accompanied with profuſe ſweatings, and often diſtinguiſhed by petechial eruptions. Cordials, and hot regimen were ſometimes obſerved to cut ſhort its courſe abruptly; yet dangerous ſymptoms were ſtill more frequently the conſequence of this ſtimulating mode of treatment, than a favourable termination. The ſagacious Sydenham, inſtructed by repeated experience of the bad effects of this common method of cure, adopted a contrary one; which he purſued with boldneſs, and apparently with great ſucceſs. It may not be improper to obſerve in this place, that our author is not to be conſidered as the inventor of the antiphlogiſtic method of treating fevers. The ancients, particularly the Arabians, carried the cooling ſyſtem ſtill farther than the moderns. About this time however it had fallen into general neglect; and Sydenham undoubtedly poſſeſſes the merit of reſtoring it; more perhaps from his own obſervation, than from a knowledge of what had been done by his predeceſſors. Part of the year 1669, the years 1670,-1671 and 1672, form another conſtitution of ſeaſons, according to this author's arrangement of diſeaſes. The epidemic aſſumed a different appearance from the former. It was chiefly diſtinguiſhed by ſymptoms of dyſenteric affection. Our author, however, ſtill adhered to the outlines of the antiphlogiſtic plan; and treated the diſeaſe ſucceſsfully with

bleeding, and the repeated uſe of laxatives. The method of treatment, which he adopted, admits of a remark. In the former epidemic, the profuſe ſweatings were checked; in the preſent, the inteſtinal evacuations were encouraged; in one caſe he appeared to promote, in the other to thwart the intentions of nature; practices ſo oppoſite that we cannot eaſily reconcile them. The next conſtitution, viz. the years 1673,-74 and 75 diſcovered a fever with a new train of ſymptoms, and in Sydenham's opinion of a very different race. It was principally diſtinguiſhed by pleuretic and rheumatic affections, by coma and ſtupor. The general antiphlogiſtic practice was ſtill perſiſted in; and the whole of the cure was truſted to diſcretional bleeding, bliſtering the back part of the head and neck, with the repeated employment of glyſters. The hotter diaphoretics were cautiouſly avoided. In the year 1684, this diligent obſerver imagined he diſcovered the appearance of a fever of a perfectly new and unknown kind; a fever accompanied with more or leſs derangement of intellect, and many other ſymptoms of nervous affection; the ſpecies of diſeaſe, perhaps, which noſologiſts have diſtinguiſhed by the name of Typhus. But though this ſpecies of fever was ſuppoſed by our author, to be extremely different in its nature from any that he had yet ſeen, we do not however perceive, that this idea ſuggeſted to him any material difference in the mode of treatment.

From the ſhort view which has been given of Sydenham's practice in fevers, it is eaſy to perceive the riſe and progreſs of the method of cure which he adopted. Antiphlogiſtic proceſſes were carried to a greater length by the ancients, than the moderns have yet dared to riſk. But there is little reaſon to ſuppoſe, that Sydenham owed the ideas of the alterations which he introduced to information from preceding writers. His practice bears every where authentic

marks of having arisen from his own observation.—The most common termination of fevers, is by sweating or increased perspiration; a fault observed by Van Helmont, and which furnished that author with the idea of prosecuting the cure of the disease wholly on this plan. The practice seems to have been early adopted in many parts of Europe; and it even continued in general reputation in England, at the time that Sydenham began his medical studies. Sweating undoubtedly is often beneficial, and may be considered, upon the whole, as the most certain means of exterminating the cause of fevers; yet bad effects often resulted from it---then probably more owing to the manner in which it was conducted, than to the real hurtfulness of the thing itself, viewed in the light of a general remedy. Sydenham, who does not appear to have discriminated between the actual effects of sweating and the effects of the manner of exciting it, condemns the practice in general terms, and passes to an opposite method of treatment with a good deal of boldness. It has ever unfortunately been the fate of physic, like every other conjectural art, to pass from one extreme to its opposite by large strides; and thus, even the sagacious Sydenham, who had seen the bad effects of treating remedies in fevers with much of the inflammatory diathesis, was induced to employ antiphlogistic processes in those species of disease, which we should be disposed to believe do not well admit of them. The new, or nervous fever, in the opinion of the practitioners of the present age, could not well bear the plentiful evacuations prescribed by this author; at least, we may safely affirm, that such evacuations are not by any means necessary.—But I shall dismiss this subject with observing, that the practice of Sydenham, if we except the article of bleeding, can only be considered as feeble, and as often insignificant. His remedies sometimes, perhaps, obviate the fatal tendency of symptoms; but are not

capable of having any decided effects on the natural courſe of the diſeaſe. I may likewiſe add, that his practice is directly at war with the principle of his theory. If fever is conſidered an effort of nature to exterminate ſomething hurtful from the conſtitution, bleeding and thoſe evacuations, which diminiſh the powers of life, are not the proper means of effecting this purpoſe. But the truth is, the practice of Sydenham was his own; his theory was that of the times in which he lived, formed from a mixture of the doctrines of Van Helmont, Campanella and Dr. Willis.

It may not be ſuſpected, perhaps, from the remarks which I have made on the practice of Sydenham in fevers, that I do not conſider him as the author of ſo much eſſential improvement, as has been generally imagined. I muſt however acknowledge, that he deſerves the higheſt praiſe for the accurate and well diſcriminated hiſtory of acute diſeaſes, which he has left us. The deſcriptions are complete, and the circumſtances ſo peculiarly choſen, that the diſeaſe itſelf is actually before the eyes of the reader. Theſe are the great, and as yet the unrivalled excellencies of Sydenham; but in admitting ſuch eſſential differences in the cauſe of epidemics as he has done, he neceſſarily leads us to embarraſſment, and often leaves the practitioner in a ſtate of uncertainty. The diſeaſe deſcribed by Sydenham, in the various conſtitutions of ſeaſons between the years 1661 and 1685, ſhews external marks of conſiderable diverſity; yet I muſt confeſs, that I ſee but little reaſon for ſuppoſing, that theſe appearances ariſe from cauſes which are totally and fundamently diſtinct. The fever of Sydenham, in all its forms, is in fact the common endemic of England. Circumſtances however often aroſe then, and ſtill ariſe, which modify the general cauſe in ſuch a manner, that the diſeaſe appears at one time with ſymptoms of inflammatory diatheſis, at another with

ſymptoms of nervous affection, and at another, with a general diſpoſition to affections of particular organs. Theſe modifying cauſes, which are more general or particular, more obvious or obſcure, often continue for a certain train of ſeaſons, and influence very materially the character of the reigning epidemic. The general cauſe of the fever is in reality one and the ſame, yet I muſt alſo acknowledge, that the modifications are evidently many and various, and often very remotely different from each other.

Chemical principles for ſome time paſt, had the principal ſhare in enabling medical writers to account for the phenomena in fevers; but about the end of laſt century, the mechanical philoſophy was again revived, and being incorporated with the doctrines of the chemiſts, the laws, and various derangements of the human frame, were then explained on the principles of hydraulics, or chemical mixture. The authors who adopted this mode of reaſoning were numerous, and ſome of them were of great eminence; but at preſent I ſhall only take notice of one of the greateſt of them, the celebrated Boerhaave, who formed a ſyſtem, which was conſidered as the moſt perfect that had hitherto been offered to the public. The doctrines of this author acquired uncommon fame. They ſoon extended over all Europe, and, indeed, ſtill prevail in the greateſt part of it. But though Boerhaave has preſented us with a methodical explanation of the phenomena in fevers; and has detailed the method of cure with clearneſs and preciſion; yet we do not find, that he has furniſhed much that is new and original in practice. He is every where cautious, and in moſt inſtances judicious; though he has committed a principal error in forming indications of cure, from a ſuppoſition of lentor and viſcidity; a cauſe the very exiſtence of which we have every reaſon to doubt.

During the time that Boerhaave flouriſhed in Hol-

land, indeed before this author had arrived at much reputation, Profeſſor Stahl, at Halle in Saxony, propoſed ſome new opinions, which acquired conſiderable fame, and which have been conſidered, in ſome manner, as forming a peculiar ſyſtem. The leading principle of this author, as is confeſſed by all, admits only of a feeble and inactive practice. I might even add, that it frequently leads to a pernicious one. Thoſe tumults, or ſufferings, which paſs by the name of the efforts of nature, are extremely deceitful; and have obviouſly, in many inſtances, a deſtructive tendency. I mentioned before that they are truſted with danger; yet Stahl, proceeding on this principle, boaſts extraordinary ſucceſs in the cure of the petechial fever, which prevailed in moſt parts of Saxony towards the end of laſt century.

In a review of thoſe authors, who have written on febrile diſeaſes, it would be unjuſt to omit mentioning Hoffman, contemporary with Stahl, and profeſſor in the ſame univerſity. The actual alterations which this author has introduced into the cure of fevers, are not perhaps very great in themſelves; yet his important diſcoveries, in regard to its theory, entitle him to great conſideration. The moſt of the remedies, which he employed, are found in the writings of his predeceſſors, or contemporaries; yet they were not, perhaps, always preſcribed by them with the ſame intentions. The theory of Hoffman opens a road for the trial of antiſpaſmodics, merely on the footing of antiſpaſmodics; a claſs of remedies of much importance in the cure of febrile diſorders. In practice, Hoffman is more decided than Stahl; and his views, perhaps are more extenſive than thoſe of Boerhaave. He is likewiſe uncommonly candid; and has furniſhed us with a great variety of hiſtories, which ſerve in many caſes to illuſtrate the nature of the diſeaſe.

The antiphlogiſtic method of treating fevers, the

ground-work of which was laid by Sydenham, and improved by Boerhaave, prevailed in moſt parts of Europe, without material alteration, till near the preſent times. Bliſtering with cantharides, which had been employed with caution, and which was even ſuſpected of deleterious effects by many, was introduced into practice in the end of the ſixteenth century, aad about the beginning of the preſent began to be, as employed, a common remedy in many ſpecies of fever: its good effects were often obvious, and, according to the prevailing mode of reaſoning, were ſuppoſed to ariſe from a quality which cantharides were believed to poſſeſs, *of attenuating* the blood. This mode of operation is no longer admitted; but the remedy ſtill retains its credit. Few people pretend that bliſters are poſſeſſed of ſpecific powers in ſhortening the courſe of fevers; yet every one allows, that they obviate many ſymptoms of dangerous tendency, and that they often diſpoſe the diſeaſe to aſſume it proper form. In fevers, accompanied with local affection, their beneficial effects are univerſally acknowledged; and, even in many caſes of general irritability, they often produce very fortunate changes. But I muſt obſerve, with regard to this, that much depends on managment, and the mode of application. In local affections the local application is moſt effectual; in caſes accompanied with much general irritability, the back part of the head and neck ought, perhaps, to be preferred to others. I have thus frequently ſeen in fevers, where there was much general irritability, that bliſters applied to the extremities evidently aggravated the diſeaſe; while I have alſo obſerved, that they as certainly diminiſhed the hardneſs and frequency of the pulſe, and diſpoſed the patient to reſt, where they were applied to the back part of the head and neck. There is another remedy that I ſhall take notice of before leaving this ſubject, which poſſeſſes ſtill higher reputation than bliſters.

Antimonial preparations have been employed occasionally in fevers for many years past; but they did not gain eſtabliſhed credit in this country, till within theſe thirty years. The diſcovery of the famous powder of Dr. James appears to have been the cauſe of a conſiderable innovation, in the manner of treating febrile diſeaſes. The practice of Boerhaave did not go farther than to obviate ſymptoms of fatal tendency; it left the diſeaſe to purſue its own courſe. Dr. James aſſumed a bolder ground, and promiſed to cut ſhort the fever abruptly by means of his powder. There are many who ſtill tread in his footſteps; I acknowledge, as I have hinted before, that their attempts may be often ſucceſsful in the early ſtages of the illneſs, or often uſeful towards a critical period. I cannot however believe, that this powder, or any preparation of antimony with which we are yet acquainted, poſſeſſes the power of abruptly terminating a fever wherever it is employed; at leaſt, to effect this requires a management of which I confeſs myſelf ignorant. The effects which Dr. James promiſed from his powder, others have attempted to obtain from emetic tartar; but I have reaſon to think with inferior ſucceſs.

The wonderful power, which the Peruvian bark is obſerved to poſſeſs, in ſuſpending the courſe of intermittents, has led the practitioners of the preſent times to employ it, with the ſame views, in fevers of various denominations. But after what I have ſaid of the uncertainty of its effects in checking the courſe of the remitting fever of Jamaica, it will be needleſs to repeat here, that I do not expect to find it of much efficacy, in ſhortening fevers of a more continued kind. I muſt, however confeſs, that, even in many of theſe, it is a remedy of great value. It ſupports, in a very eminent degree, the tone and vigour of the powers of life.

Opium has been preſcribed occaſionally in fevers

for a long time paſt; but it is only of late years, that it has been recommended, as a general remedy in ſome particular ſpecies of this diſeaſe. The practitioners of the Weſt Indies, preſcribe opium with more freedom, than is generally done in England. It is frequently employed to mitigate ſymptoms; and in ſome ſituations which were very alarming, I have given it in very large quantity with unexpected good effects. In the ſlow fevers of this country I have frequently had recourſe to it; and, combined with antimonials and camphire, have found it to be a remedy, of great value. Opium in general was more cordial than wine. In caſes of deſpondence and diſtreſs it gave a confidence to the mind, and imparted a pleaſureableneſs to the ſenſations above all other remedies. In ſhort, it appeared often, not only to be inſtrumental in conducting the diſeaſe to a favourable termination, but it enabled the patient to paſs through it with comfort to himſelf.

I have mentioned in the preceding pages, the moſt eminent of thoſe authors, who have written on the cure of fevers; giving at the ſame time ſuch extracts from their works, that the reader, who has not the opportunity of conſulting the originals, may be enabled to form ſome idea of the ſucceſſive changes, the improvements, and oftener perhaps the corruptions, which have ariſen in the method of treating febrile diſeaſes, from the earlieſt records of the art to the preſent times. The apparent changes are more numerous than the real ones; while the moſt oppoſite modes of treatment do not often appear to have much perceptible effect on the event. The cure of fever has been hitherto purſued on two general and oppoſite views, viz. on the idea of exciting the powers of life, by means of heating and ſtimulating remedies; or of diminiſhing the reaction of the ſyſtem by evacuations and other antiphlogiſtic proceſſes. The above extremes of thoſe directly oppoſite modes

of treatment have approached gradually to each other, or been variously combined by different practitioners. It cannot however fail of appearing strange to a person, who views the science of medicine in a philosophical light, to hear one set of men asserting that the proper cure of fever consists in exciting the powers of life, or in enabling nature to expel the disease by force; while another, with no less confidence, maintains that the plan of moderating or diminishing increased action is that which ought alone to be pursued. From such contradictory assertions we cannot easily avoid concluding, either that the most opposite means produce the same effect, or that nature has a prescribed mode of proceeding in fevers, which ordinary medical assistance is not powerful enough to controul. There are many eminent practitioners, who have been conscious of this truth. The candid Sydenham himself acknowledges, that those, whom he treated with all his skill and attention, and who possessed all the comforts that affluence could afford, did not often fare better than the poor, who were only sparingly furnished with necessaries, and who met with little assistance from medicine. I have myself seen many examples of the same kind. Sometimes I pursued the usual methods of cure with care and perseverance; sometimes I left the business almost entirely to nature, and I cannot say, that the difference of the event gives me much cause to be vain. But though I may appear to be sceptical with regard to the effects of common practice, I still cannot help being of opinion, that we may arrive at a high degree of perfection in the management of febrile diseases. So sanguine, indeed, are my expectations, that I cannot easily forgive myself, when the event of this disease happens to be unfortunate. The remitting fever of Jamaica is not a disease by any means devoid of danger; yet I should not be satisfied with myself, from the view which I now have of the subject, if I

loft one patient in fifty. I own indeed that this is a degree of fuccefs, which neither I, nor perhaps any other man has yet attained. I muft however add, that I have not always had the liberty of doing what I wifhed to do; neither have I always dared to venture upon what I judged not proper to be done. The prejudices of patients in fome cafes, and the idea of refponfibility in others, confine us to the beaten track, though we may be conficious in ourfelves that it never can lead us to our object. If thefe obftacles were removed, a man who will act with decifion, may promife almoft any degree of fuccefs in the remitting fever of the Weft-Indies, in conftitutions which are free from habitual complaints.

The conftant fluctuation which has hitherto prevailed in the opinions of phyficians concerning the caufes of fever, and in their practices with regard to its cure, oblige us to think doubtfully of the real progrefs of the healing art. Hippocrates was allowed to have practifed with more fuccefs than his predeceffors. Afclepiades was believed by many to have been ftill more fortunate than Hippocrates; yet the road which he purfued was totally different. Galen, who reviewed and improved the fyftem of the Coan fage, rofe to great eminence, and marked out the path of medical practice for many centuries. The doctrines of Paracelfus fhook his authority; and thefe in their turn gave way to newer modes of thinking. In this manner there have been fuch perpetual revolutions in the modes of treating febrile difeafes, that we can fcarcely avoid concluding, that little or nothing of the matter is yet known with certainty. Medical writers have wandered from conjecture to conjecture, for more than two thoufand years; and we do not yet perceive any profpect of thefe conjectures being nearer to an end.

APPENDIX.

CONTAINING SOME HINTS WITH REGARD TO THE MEANS OF PRESERVING THE HEALTH OF SOLDIERS SERVING IN HOT CLIMATES.

HAVING treated pretty fully of the remitting fever of Jamaica, and intermitting fever of America, I shall now offer a few thoughts on the various means of preserving the health of soldiers in warm climates; taking the liberty at the same time to suggest some ideas, which might perhaps be usefully attended to by those who superintend the medical establishments of the army.

The climate of the West-Indies has been fatal to the European constitution, even since its first discovery by Columbus. To the armies and navies of England it has been particularly destructive. The sad fate of the troops who went on the expedition to Carthagena will be long remembered; neither will the loss sustained at the Havannah, Martinique and Gaudaloupe soon be forgotten; while the destruction, occasioned by the effects of climate at St. Lucia, St. Juan, and even in Jamaica, during the late war, is still fresh in our memories. As it appears from a comparative view of the mortality of the troops employed in these different services, that we have profited but little by the experience of our former misfortunes, it might probably be supposed, that the great sickness, observed on these occasions, has actually arisen from the irremediable effects of climate, or unavoidable hardships of service in hot countries; but there is reason to believe that this is not wholly the case. I will venture to assert, nor should I expect to meet with difficulty in proving, that much

of it has proceeded from the inexperience or inattention of thoſe who conducted the expeditions, or from ſuch errors in the medical departments as might have been eaſily obviated. It is ſuperfluous to obſerve, that the health of the ſoldier is an object of principal importance in enſuring the ſucceſſes of war. We have many inſtances of expeditions apparently well concerted, which have failed from the exceſſive ſickneſs of the troops: and too many proofs of this ſickneſs proceeding from a neglect of ſuch precautions, as might have contributed to the preſervation of health. I have accuſtomed myſelf to look at this ſubject for more than fifteen years. I have turned it often in my mind, and cannot diſcover that much judicious attention has yet been paid to it. We cannot often perceive that health has been an object of conſideration, in fixing the permanent ſtations of troops; or that it has been much regarded in chooſing encampments in the field. Exerciſes, which might inure the body to hardſhips, have not been ſufficiently enforced; and ſuch ſorts of diet, and ſuch modes of life, as might obviate the danger of diſeaſes, have been little attended to; while the beſt regulations for a ſpeedy and deciſive plan of cure do not appear to have been adopted. I ſhall be obliged, in tracing this ſubject, to advance ſome ideas which are contrary to the opinions of ſome celebrated authors, which combat popular prejudices, or which interfere with the views of intereſted men. I may be reckoned preſumptuous perhaps in cenſuring freely; but I am conſcious that I do not advance any thing which has not truth for its foundation.

It has frequently been the practice, in times of war, to ſend new raiſed regiments to ſerve in the iſlands of the Weſt-Indies; and though the injudiciouſneſs of the practice has long been diſcovered, it does not yet appear to be diſcontinued. During the late war there were ſeveral corps ſent out to thoſe

countries newly recruited, the conſequence of which was, that though not a man died by the ſword; yet in the ſhort ſpace of two years, there ſcarcely was a ſoldier left. A great part of this dreadful mortality undoubtedly aroſe from the climate; yet ſome ſhare of it ſeems likewiſe to have proceeded from the particular circumſtances of raw undiſciplined troops.—Men newly enliſted in England, are generally of groſs and full habits, and too often accuſtomed to irregular and diſſipated modes of life. Under ſuch circumſtances, a ſudden tranſition to a hotter air, joined with full meals, and the habitual indolence of a paſſage at ſea, cannot fail to produce a plethoric ſtate of the body, which is often rendered dangerous by the incautious uſe of ſtrong liquors, or the ordinary exertions required in performing military exerciſes, under the influence of a powerful ſun. I do not pretend to inſinuate that thoſe are the cauſes of remitting fever, but I am very ſenſible at the ſame time that they are cauſes which occaſionally aggravate its danger, and which even ſometimes accelerate its appearance. In ſoldiers who have been inured to a military life, ſuch change of climate operates with diminiſhed effect. The bulk of the fluids is perhaps diminiſhed by a continuance of leſs full living; while the tone and elaſticity of the moving powers are increaſed by habits of exerciſe and exertion.—The diſpoſition to commit exceſſes is likewiſe repreſſed by the rigour of diſcipline; and the mind acquires a philoſophical firmneſs from long ſervice, which not only contributes to the preſervation of health, but which enables the individual to ſuſtain with fortitude the attack of diſeaſes.

In paſſing from a cold to a hot climate, the firſt thing that occurs to be conſidered, is the effect produced by the ſimple increaſe of heat on the human frame. Expanſion of the fluids, and conſequent fulneſs of the veſſels is conſtantly obſerved to take place

from such a change, frequently however accompanied with diminished energy of the moving powers, particularly where heat is combined with dampness of the air. To obviate therefore this natural effect of heat is the first general object to be attended to, in transporting troops to the tropical climates. The English, from the constitution of their bodies, and still more perhaps from their manner of living, suffer more from those sudden changes than some other European nations. The French and Spaniards are not only less gross constitutionally, but eat likewise less animal food, and drink their liquors greatly more diluted, than the natives of England. They do not probably owe more to medical assistance than the English; yet they are known to escape better from dangerous diseases; and their safety I might add has been remarked to bear some proportion to the different degrees of abstemiousness, which they are known to observe. An idea prevails with the generality of people, who visit warmer or more unhealthy climates, that it is necessary to eat and drink freely, as a security against the attacks of endemic fevers; but a very narrow observation will serve to shew, that good living, as it is called, has no such effects; and we may even soon perceive, unless blended by long established prejudices which flatter our appetites, that it actually is attended with pernicious consequences. The most abstemious, so far as I have observed, escaped the best, not only from the attacks, but particularly from the danger of diseases. With regard to the diet of a soldier, serving in a hot climate, I should be disposed to believe, that one spare meal of animal food would be perfectly sufficient in twenty-fours: and if it were easy to alter established customs, it would be most proper, perhaps, that it were made in the cool of the evening. Coffee, or tea for breakfast might likewise be substituted with advantage in place of the ordinary allowance of rum:

but this I must confess would be a dangerous experiment. Our soldiers have been so long accustomed to consider this gratuitous allowance of rum as their right, that no man could answer for the consequences of with-holding it. The practice certainly is pernicious, and the man, who first introduced it into the army, did no good service to his country. I do not deny that a judicious use of spirits might be of benefit occasionally: neither do I pretend to say, that, even the hardest drinking can be considered as a general cause of fevers; but it would not be difficult to produce evidence, that hard drinking aggravates the violence, and increases the danger of the disease, when it happens to take place; while I cannot perceive much reason for concluding, that the use of spirituous liquors has ever been productive of general good to the army, particularly in warm climates. But as I have just mentioned, that spirituous liquors have little claim to be considered among the number of those things, which contribute to the preservation of health: so I may add, with perfect confidence, that the allowance of rum granted to soldiers, has done much harm by ruining discipline, and good behaviour. If it is with-held for one day, discontent immediately begins to shew itself among the men. If with-held for any length of time, complaints sometimes rise to a state of mutiny, and desertions become numerous. But besides this, that soldiers seldom perform extra-duty with alacrity, unless they are bribed with a double allowance of liquor. A double allowance, drank undiluted, as is generally the case, is frequently sufficient to produce some degree of intoxication. I need not mention the disasters to which an intoxicated army is exposed. Disasters of a very serious nature have actually happened from this cause, and they might have happened oftener had the enemy been always vigilant, and bold enough to have seized the opportunity.

A deal might be ſaid on the ſubject of abſtemiouſneſs. Moderation both in eating and drinking is eſſentially neceſſary to the health of troops newly arrived in hot climates; but a truth ſo obvious need not be enforced by many arguments. The example of the French and Spaniards afford a very convincing one. It is known to every medical perſon, that the fevers of hot climates are generally moſt dangerous in full and plethoric habits. It ought to be an object of attention therefore to obviate this cauſe of mortality, by means of ſpare living, and the cautious uſe of ſtimulating liquors: but ſoldiers have little ſelf command, and ſeldom reſiſt the gratification of their appetites. Hence it becomes the duty of their officers to enforce their compliance with what is proper, and to preclude them, as much as is poſſible, from the means of obtaining what is pernicious; but this requires great vigilance and attention, and often great ſeverity. It is not enough that ſoldiers are obliged to eat in meſſes. The officers ought daily to inſpect their meals, and inflict penalties where they obſerve tranſgreſſions. And further, as it is a matter of much importance to preſerve troops in a ſtate of health fit for action, and as the courſe of fevers is often uncommonly rapid in the Weſt-Indies, it would be proper, perhaps, that the ſurgeon reviewed the men daily. The diſtant approach of the diſeaſe would be frequently diſcovered by this means, and the danger of it might probably be ſometimes averted by timely aſſiſtance. Before men appear in the ſick-reports, the fever is often conſiderably advanced in its progreſs.

Beſides the alterations which might be made in the diet of troops, on their arrival in hot climates, ſome changes in the mode of cloathing might, perhaps, be likewiſe adopted for the ſake of eaſe and convenience, if not for purpoſes of real uſe and economy. Round white hats would be the moſt proper covering for the head; and dowlas might be ſubſtituted with advan-

tage in room of the thick cloth, of which the coats of ſoldiers are uſually made. There can be no grounds for ſuppoſing, that a ſoldier will not fight as well in dowlas as in ſcarlet; and there is certain proof that he will perform duties, which require exertion, with greater ſafety and effect, as the nature of his cloathing will preſerve him cooler by ſome degrees. But though ſuch alterations may be hinted, there is little room to believe that they will be attended to. In the preſent rage for military ſhew, it will be a difficult taſk to convince men to lay aſide an uniform, which adds ſo much to the brilliancy of the appearance. Much ſtreſs ſeems at preſent to be laid upon the dreſs of the ſoldier, and I do not pretend to argue, that it is a matter of perfect indifference. It has certainly very often had viſible effects upon the enemy; but theſe effects have oftener proceeded from a knowledge of the character of the troops who wore it, than from any thing formidable in the uniform itſelf. But to leave this ſubject of dreſs, I ſhall only obſerve, that a flannel or cotten wrapper would be more uſeful to a ſoldier, ſerving in the Weſt-Indies, than a blanket; and perhaps the expence of it would not be much greater. It would ſerve for his covering in the night, and would ſecure him againſt the effects of cold, where occaſions obliged him to go out.

I ſhall endeavour in the next place to point out ſome of thoſe benefits, which may be derived to health, from habits of daily exerciſe. This is an object of the greateſt importance, but unfortunately it is an object very little attended to in the Britiſh army. It appears, indeed, to be little regarded in moſt of the armies of modern Europe. I ſhould incur a charge of preſumption, perhaps of ignorance, did I attempt to point out the exerciſes which are the moſt proper for the forming of ſoldiers. Thoſe only which contribute to the preſervation of health, belong to this place. I may however remark, that the eſſential part of the

art of disciplining troops, consists in imparting sentiments of heroism and virtue to the minds of the men, in improving the exertions of their limbs, and in acquiring knowledge of the correspondence of their exertions when called into action. If I durst take so great a liberty, I should be inclined to say, that our ordinary exercises are flat and insipid in their nature; that they occasion no exertions, and excite no emulation: they neither improve the active powers of the body, nor inure the soldier to bear fatigue and hardship. The Romans, who owed more to the discipline of their armies than any nation on earth, were extremely rigorous and persevering in their exercises. They practised their soldiers in every species of service that might occur; so that nothing at any time happened with which they were unacquainted. Actual war was in reality a time of relaxation and amusement to the soldiers of this warlike people, who appear to have been trained for the service of the field, as horses are for hunting or the course. The Romans were not only sensible of the advantages which those habits of exercise procured them in action; but had also the penetration to discover, that they were eminently serviceable in the preservation of health. The words of Vegetius are remarkable. "Rei militaris periti, plus quotidiana armorum exercitia ad sanitatem militum putaverunt prodesse, quam medicos." I made the same remark during the time that I attended a regiment in America, without knowing that it was supported by so great authority. I observed, when the men were in the field, sometimes even complaining of hardship and fatigue, that few were reported in the list of the sick: when removed to quarters, or encamped for any length of time in one place, the hospital was observed to fill rapidly. This observation was uniformly verified, as often as the experiment was repeated.

An idea has been long entertained, that the Euro-

ropean conſtitution cannot bear hard labour in the ſun, or perform military exerciſes with ſafety, in the hot climates of the Weſt-Indies. Hence a plan has been ſuggeſted, and in ſome degree I believe adopted, that regiments ſerving in thoſe countries, be furniſhed with people of colour to do the drudgery of the ſoldiers. But this appears to be an innovation which ought to be admitted with extreme caution. It will evidently ſerve to increaſe ſloth and idleneſs, and unleſs the perſons of colour can perform the military duty in the field, their ſervices will go but a ſhort way in preſerving the health of the troops. A ſoldier, notwithſtanding he may have received the King's pay for twenty years or more, remains in ſome degree a tyro till his body has been inured to fatigue, and prepared to bear without danger the effects of the climate, in which he may be deſtined to ſerve. This is a part of the military diſcipline, indeed, no leſs neceſſary than a knowledge of the uſe of arms; and though it is a part of it, difficult to be accompliſhed, there is ſtill room to believe, that it may be effected, even in the ſo much dreaded climate of Jamaica. It is a common opinion, that the fatigues of an active campaign in the Weſt-Indies, would be fatal to the health of the troops; but the opinion has been aſſumed without fair trial. The exertions of a ſingle day have often been hurtful. This was frequently the caſe in America, where the ſoldiers had remained for ſome time in a ſtate of reſt; but bad effects from the greateſt exertions, in the hotteſt weather of ſummer, were extremely rare in that country, after the campaign had been continued for a few days. But that I may not ſeem to reſt an opinion of ſo great importance on a bare analogy, I ſhall beg leave to obſerve, that young European planters undergo greater fatigues, and remain daily expoſed for a longer time to the heat of the ſun, than would fall to the lot of ſoldiers in the actual ſervice of the

field. I might likewiſe further confirm the opinion, that an Engliſhman is capable of ſuſtaining fatigue in the Weſt-Indies, equally well with the African, or the native of the iſlands, by mentioning a journey which I once performed myſelf. I lived about four years in Jamaica, during the greateſt part of which I believed that death, or dangerous ſickneſs, would be the conſequence of walking any diſtance on foot; but I afterwards learnt that this apprehenſion was vain. I left Savanna la Mar in the year 1778, with the deſign of going to America; but having embarked in a hurry, and forgot a material piece of buſineſs, I found a neceſſity of being put aſhore, after having been two or three days at ſea. I was landed at Port Morant, in St. Thomas's in the Eaſt, from which I went to Kingſton by water, where learning that there was a veſſel at Lucca, in the Weſtern extremity of the iſland, nearly ready to ſail for New-York, I ſet out directly, that I might not loſe the opportunity of a paſſage. My finances not being in a condition to furniſh horſes, I left Kingſton on foot, about twelve o'clock, and accompliſhed a journey before it was dark of eighteen miles. I did not find I was materially fatigued and ſtill perſiſting in my reſolution, travelled a hundred miles more in the ſpace of the three following days. It may not be improper to remark, that I carried baggage with me, equal in weight to the common knapſack of a ſoldier. I do not know that ſo great a journey was ever performed on foot by an European, in any of the iſlands of the Weſt-Indies; not ſo much I am convinced from inability, as from idea that ſuch exertions are dangerous. But as it appears from the above fact, that the European conſtitution is capable of ſuſtaining common military fatigues in the climate of Jamaica; ſo I may add that it ought to be a principal object of military diſcipline, that ſoldiers be practiſed with frequent marching, and the performance of

other exercises of exertion, if it is actually meant that they should be useful in times of war. The fate of battles, I might observe, depends oftener on rapid movements, in which the activity of the limbs is concerned, than on the expert handling of arms, which is acquired by the practice of the manual. I observed formerly, that abstemiousness and temperance were among the best means of preserving health, or obviating the danger of the diseases to which troops are liable on their first arrival in hot climates; but the rules of temperance are little regarded by English soldiers at any time, and almost constantly transgressed wherever extraordinary labour is required of them. To such causes of excess, joined with the great heat of the sun, we may perhaps impute many of the bad effects of marching, or of moderate fatigue in the West-Indies. In the journey which I have just now mentioned, I probably owe my escape from sickness to temperance and spare living. I breakfasted on tea about ten in the morning, and made a meal of bread and sallad, after I had taken up my lodging for the night. If I had occasion to drink through the day, water or lemonade was my beverage. In the year 1782, I walked between Edinburgh and London in eleven days and a half; and invariably observed, that I performed my journey with greater ease and pleasure, where I drank water, and only breakfasted and supped, than when I made three meals a day, and drank wine, ale, or porter. In the following summer I carried the experiment farther. During the months of July and August, I travelled in some of the hottest provinces of France. I generally walked from twenty-five to thirty miles a day, in a degree of heat less supportable than the common heat of Jamaica, without suffering any material inconvenience. I breakfasted about ten o'clock on tea; coffee or syrup of vinegar, made a slender meal of animal food in the evening, with a great proportion

of ſallad and vegetables; but never drank the weakeſt wines without dilution. The great refreſhment which I found from ſyrup of vinegar and water, convinces me, that the Romans had good cauſe for making vinegar ſuch an eſſential article among the proviſions of their armies.—The ſtate of luxury and our depraved appetites, unfortunately do not ſuffer it to be adopted by the Engliſh. I ought perhaps to make an apology to the reader for introducing my own experience on the preſent occaſion: but I muſt add, that I have only done it, becauſe it enables me to ſpeak from conviction, that an Engliſh ſoldier may be rendered capable of going through the ſevereſt military ſervice in the hotteſt iſlands of the Weſt-Indies, and that temperance will be one of the beſt means of enabling him to perform his duty with ſafety and effect.

I mentioned before, that the military exerciſe of the Engliſh army is ill calculated to excite a ſpirit of emulation among the men. It is in fact conſidered only as a piece of drudgery, in which there are few who have any ambition to excel. It has little effect in improving the activity of the limbs, or hardening the conſtitution of the body; ſo that it may better ſuſtain hardſhip and fatigue. But feeble as its effects are in the view of increaſing exertion, or preſerving health, it is generally almoſt intirely diſcontinued when troops arrive in hot climates; a practice, which has ariſen from a ſuperficial and miſtaken view of the ſubject. Sloth and indolence are the bane of a ſoldier in every climate; exerciſe and action are the greateſt preſervatives of diſcipline and of health. It would be reckoned preſumption in me, and it does not belong to this place to point out thoſe exerciſes which might be proper for the forming of ſoldiers. But every one knows that walking, running, wreſtling, leaping, fencing and ſwimming, are often called into actual uſe in the practice of war. Theſe are

ſuch exerciſes likewiſe as excite emulation, and are practiſed with pleaſure by the individual. They harden the body, increaſe the power of the limbs, and by furniſhing the officer with a view of the different degrees of activity, may often enable him to place his men in the ranks, according to the uniformity of their exertions; a more uſeful mode of arrangement in time of action, than uniformity of exterior form. I may add in this place, that ſea-bathing will be extremely uſeful in moſt caſes, in increaſing the vigour and preſerving the health of ſoldiers ſerving in warm climates. There no doubt will occur many caſes, in which it is improper; but in general it may be employed with great benefit. I chiefly impute it to this cauſe, that I did not experience a ſingle day's indiſpoſition, during the four years that I lived in Jamaica.

It has been known for many ages, that the cauſe of intermitting and remitting fevers, the moſt formidable diſeaſes of hot climates, owes its origin to exhalations from ſwampy and moiſt grounds. It often happens likewiſe, that thoſe low and ſwampy grounds are the moſt acceſſible parts of a coaſt, or that towns and ſettlements have been placed near them—to attack or defend which falls to the lot of the ſoldier. It not being therefore in the power of a military commander to remove the natural diſadvantages, which I have mentioned; it is only in his power to ſhew his judgment and attention, by applying the beſt remedies to obviate their effects. It is certainly an object of the utmoſt conſequence to preſerve troops in a ſtate of health fit for action: and no perſon will deny, that every care ought to be employed in chooſing the beſt ſituations for quarters, or even temporary encampments, that the nature of the duty will permit. We learn from experience that fevers are little known in rough and hilly countries, where water flows with a rapid courſe; while we likewiſe

know, that they are common in low and champaign countries, where water ſtagnates, or has only a ſluggiſh motion: independent of which, thoſe ſituations which are in the neighbourhood of ſwamps, or near the oozy banks of large rivers, have always been obſerved to be particularly liable to ſuch diſeaſes. If therefore the circumſtances of the ſervice do not forbid, no room can be left to doubt about the propriety of ſtationing troops in the mountanous or hilly parts of a country; while I may likewiſe add, that where neceſſity confines them to the plain, the ſea ſhore will in general be found to be the moſt eligible. But beſides the above general character of local ſituations there are likewiſe ſome ſubordinate circumſtances, which deſerve to be particularly attended to in chooſing the ground of encampments. It is very commonly believed that high and elevated ſituations are the moſt uniformly proper for this purpoſe; but this in fact is not, by any means, a general rule. A high and dry ſituation does not contain any thing hurtful in itſelf; but it is more than others expoſed to the effluvia which are carried from a diſtance. It is the peculiar nature of exhalations to aſcend as they proceed from their ſource; in confirmation of which truth I have had ſeveral opportunities of witneſſing, that this cauſe of diſeaſe was carried to riſing grounds in a ſtate of great activity; while it apparently paſſed over the plain or vallies which lay contiguous, without producing any material effects. From the knowledge of this fact we are furniſhed with this obvious remark, that it will be proper to interpoſe woods or riſing grounds to the progreſs of thoſe noxious vapours; or where ſuch natural advantages do not exiſt, it might be ſerviceable to burn a chain of fires in a temporary encampment, or even to raiſe a parapet wall to over top the barracks, where neceſſity requires a more permanent ſtation.——It would be a matter of utility, could we determine with any cer-

tainty to what distance from its source, the noxious effluvia extend; but this is a question which we cannot hope to ascertain very exactly. It is not uniformly the same in all situations, depending on the concentrated state of the exhalation at its source, the obstacles it meets with in its progress, and the nature of the ground over which it passes, or to which it is directed. I have known its influence very remarkable at the distance of a mile and a half, on the top of a hill of very considerable elevation.

The conveniences of trade have often tempted colonists to place their towns on the banks of rivers, without regard to the healthfulness of the situation.--The choice of such spots, injudicious as it evidently is, has been greatly approved of, and warmly recommended as preferable to others for the encampment of troops, by a very celebrated medical authority. Sir John Pringle considers the banks of large rivers as extremely proper for this purpose, on account of a free circulation of air; but I am sorry to observe, that Sir John Pringle's opinion on this occasion appears to have arisen from his theory, rather than that his theory has arisen from observation. We have actual experience of the unhealthfulness of the muddy banks of large rivers in hot climates; and we have little cause to dread diseases, which originate from confined air in America, the West-Indies, or perhaps in any country where troops are employed in the field.

I have just now observed, that the banks of large rivers, in the opinion of Sir John Pringle, afford the most eligible situation with respect to healthiness for the encampment of troops. I may add, that the same author has likewise recommended open grounds for this purpose, in preference to woods; and that the same favourite idea, viz. a free circulation of air, has influenced his advice. I will not contend, that open, dry and cultivated grounds may not be preferable to grounds covered with wood, where the heat

of the climate is moderate; but I have no doubt in aſſerting, that encampments on lands, the woods of which have been newly cut down, as is generally the caſe in times of war, are of all others the moſt unhealthful. I have myſelf ſeen ſeveral examples of it. Perhaps it is in a great meaſure owing to this cauſe, that new countries are generally ſo fatal to the firſt ſettlers; as alſo, that troops ſuffer ſo remarkably in carrying on the ſieges of places which are ſurrounded by woods: it being conſtantly obſerved, that effluvia from moiſt lands, when firſt expoſed to the action of a powerful ſun, are always highly pernicious. The Romans, whoſe obſervations on ſubjects which relate to war, may be oppoſed with confidence to the authority of the moſt celebrated moderns, were fully ſenſible of the advantages of encamping under the ſhelter of wood. We learn from Vegetius, that their armies reſorted to the cover of a wood, not leſs carefully, than that they avoided the vicinity of ſwamps or marſhes. There are in reality various circumſtances, which contribute to render ſuch ſituations both healthy and agreeable. If troops are encamped in the body of a wood, the noxious effluvia, which may be carried by the winds from neighbouring ſwamps, are ſtopt in their progreſs; the lofty ſhade of the trees preſerves the air cool and more refreſhing than the atmoſphere of the open country; while we know from experience, that moiſt and ſwampy lands do not ſend forth their noxious vapours, in any remarkable degree, unleſs where they are acted upon by the heat of a powerful ſun.

I ſhall only further obſerve, with regard to the cauſe of intermitting and remitting fevers, that a ſpace of time almoſt conſtantly intervenes between expoſure to the noxious effluvia, and the ſubſequent appearance of the diſeaſe. It is not indeed uniformly the ſame in all caſes, appearing to depend not only on the concentrated ſtate in which the exhalation is

applied to the body; but on the general aptitude of the individual, and the various occasional or exciting causes, which facilitate or resist its operation. It was in a few instances only, that I saw the disease appear before the seventh day. It was oftener the fourteenth, twentieth, or even longer. Upon the whole I may remark, that septenary periods has a considerable power in influencing the time of its appearance.

Having offered a few observations in the preceding pages, on the diet, exercises and choice of the quarters or encampments for troops in hot climates; I shall now add a few hints respecting medical care and management. It will probably be supposed, that no attention with respect to this subject has been omitted. Regiments are provided with surgeons, and armies have always been furnished with ample hospital establishments. But this perhaps is not enough. It is necessary that the duties of these stations be well executed, as well as well designed. The office of surgeon to a regiment is an office of trust and of primary importance; the appointment to it, however, does not seem in general to be sufficiently attended to. The surgeoncies of regiments, till lately, were allowed to be bought and sold; in consequence of which abuse, little other qualification, came to be required, than the command of the purchase money. Thus it often happened, that young men, who had attended a course of anatomical lectures, or walked the rounds of an hospital for a few months, came at once to be entrusted with the care of the lives of six or seven hundred soldiers, who, as they are raised and maintained at a great expence, deserve, on the score of economy, independent of every other consideration, to be well taken care of. It would be superfluous to use any arguments to prove the prodigality of committing the care of a regiment to men, who have not had professional experience in any country, and who are totally unacquainted with the diseases of the countries

to which they are frequently ſent. If we are diſpoſed to believe that there is any thing in medical treatment, we can ſcarcely avoid making the concluſion, that many lives are loſt from this cauſe. It muſt not be underſtood, that I mean any thing diſreſpectful to the ſurgeons of the army, by this inſinuation. I know that a regiment is an excellent ſchool for medical knowledge; and that the beſt practitioners have occaſionally appeared in the army; but I wiſh ſtrongly to inculcate the propriety of obliging candidates for this office, to produce evidence of their qualifications, before they are admitted to ſuch an important truſt. It is not enough, that a young man, who offers himſelf to take charge of the health of a regiment, ſhould know to perform an operation with dexterity. Handling a knife in reality is the leaſt part of a regimental ſurgeon's duty. The office of phyſician is his daily employment, to execute which properly, both years and experience are required. It certainly ought therefore to be an object of concern with thoſe who are entruſted with the office of ſuperintending the medical appointments of the army, that the candidates for ſurgeoncies be obliged to ſubmit to ſuch trials, as may in ſome degree afford proofs of their abilities. It would be a proper regulation, perhaps, that no man be permitted to propoſe himſelf for the ſurgeoncy of a regiment, before he has arrived at ſuch an age, as may have furniſhed him with general experience; and further, that he give teſtimony of actual abilities by the treatment of diſeaſes in an hoſpital, under the inſpection of an able phyſician, to whom the duty will be preſcribed to examine the mode of practice with rigour. A trial of this ſort might be better truſted to than the recommendatory letter of a profeſſor; or even the diploma of Oxford or Edinburgh. There is not any thing chimerical in the propoſal. Nothing in ſhort is more practicable; but it is ſcarcely to be expected, that men of talents and education will give

themſelves ſo much trouble, that they may be admitted into a ſervice which holds out few advantages. The ſalary of regimental ſurgeons is ſmall; and it is perhaps no paradox to ſay, that this is a cauſe of great expence to the nation. The bare ſubſiſtence, which the ſervice affords, furniſhes no inducement for men of abilities to enter, at leaſt to remain any length of time in the army, which unfortunately has been conſidered in no other light, than as a place, where ſurgeons may paſs their noviciate; but which they are generally diſpoſed to leave, as ſoon as they are qualified to execute the duty properly. Medical knowledge is gained only by experience but independent of medical knowledge, an acquaintance with the habits, characters and diſpoſitions of ſoldiers is a matter of ſo great importance, that old ſurgeons, even of inferior abilities as medical men, have generally been obſerved to have a proportionably ſmall liſt of ſick in their reſpective corps. The qualities that are principally required in a regimental ſurgeon, excluſive of medical learning and knowledge, are acuteneſs in diſcerning the characters and diſpoſitions of men, and above all, boldneſs and deciſion in the application of remedies. Life is often loſt in unhealthy climates, by the dilatorineſs and timidity of common practice.

Having mentioned juſt now, that there appears to be a remiſſneſs in examining the qualifications that are requiſite for the office of regimental ſurgeon, I might perhaps, with equal juſtice, extend the remark to the appointments in the general hoſpital.—The power of appointing phyſicians or ſurgeons in the hoſpital, has generally been lodged with the commanders in chief, and I might ſay, without tranſgreſſing the bounds of truth, that merit has not always been the beſt claim for promotion. It would be invidious to be more particular in cenſuring what is paſſed. It is only hoped, that the ſubject will be enquired into, and ſuch remedies applied, as will pre-

clude similar abuses in future. The general hospital has ever been a heavy article in the expences of war; and if it were fair to form an opinion of the whole, from the part which I have seen, I should not hesitate in declaring, that the establishment is in a great measure superfluous. I have no doubt, in obtaining the suffrages of people of experience, that general hospitals are ruinous to military discipline; that they promote sloth and indolence, the worst disease to which a soldier is liable, and that they extinguish very speedily the ardour for the service of the field.--There is in fact no exaggeration in the assertion, that the man, who has spent two or three months in a general hospital, is less of a soldier than when he was first recruited. It is only I may add by habits of exercise, even by toils and fatigue, that men at last attain the properties of good soldiers: while it is only by constant practice of such discipline, that they are preserved in a state fit for the performance of their duties. These active qualities are speedily extinguished by the habits of sloth and indolence, which prevail in general hospitals; but besides this, it is likewise certain, that cures are often there protracted to months, which might have been accomplished in the course of a few days, if circumstances would have permitted the men to remain with their regiments.—Regimental surgeons have many inducements to exert themselves in restoring their men speedily to health, which act only with feeble power on those who have the management of general hospitals. The former likewise possess some advantages, of which the latter are destitute. They know the habit and dispositions of the patient; they see the disease in its first beginnings, and are enabled to seize the most favourable moments for acting with decision. The above are considerations, which ought to make us backward in removing sick soldiers to general hospitals; I may add, that such is the nature of military diseases, that

there does not, perhaps occur one case in twenty, which might not be treated properly by the surgeon of the regiment, if attention, and a very little expence were bestowed in providing necessary accommodation. But besides that, the diseases of soldiers are seldom of such a kind, that they might not be treated properly by regimental surgeons, if government were at the expence of supplying a few conveniencies.

I may farther observe, that together with the indolence naturally attached to general hospitals, and uniformly hurtful to military discipline, there is often actual danger to life, by removing men in critical situations, or by the necessary intermission of medical assistance, where continual and vigorous exertions are required The diseases of hot climates, particularly the fevers of the West-Indies, are often most acute and rapid in their course. The surgeon of a regiment perceives the approach of danger, and, sensible that his situation does not enable him to do justice to his patient, determines to remove him to the general hospital. But time is lost before this can be accomplished. It is seldom that any thing is done after it is deemed proper to send a sick man away; neither does it commonly happen, that any thing material is attempted on the day on which he is received. Thus one day at least, sometimes two are completely lost in cases, where every moment is of consequence. Time is precious in the fevers of hot climates; and the decision or neglect of an hour often determines the fate of a patient.

It is an observation, which cannot fail of having frequently occurred to people who have served any time in the army, that it would be a very great advantage to the service, if sick soldiers could always be taken care of by the respective surgeons of the regiment. I have endeavoured to shew, that the plan is practicable and easy; and I may further add, that the expence necessary for such an arrangement,

would not amount to one third of what is uſually ſpent in general hoſpital eſtabliſhments. If this idea were adopted, nothing more would be required, than that proper lodgings, proper proviſions, and a ſufficient ſupply of medicines, were furniſhed for the ſick; that the ſurgeon of the regiment be well qualified for his ſtation; and that an inſpector be appointed for a certain portion of troops, to take care that the duty be well and diligently executed. By this means a general hoſpital, as far as regards medical treatment, might be aboliſhed, or at leaſt greatly abridged. Where fighting was expected, extra-ſurgical aſſiſtance would ſtill be neceſſary. Such an inſpection of regimental hoſpitals, as that I have mentioned, ſeems to be perfectly ſufficient for the care of the health of an army, in ordinary occaſions. The greateſt precautions, however, ought to be taken, that the inſpection does not degenerate into a nominal duty. The inſpector ought to viſit the different quarters, examine minutely into every article of the management of the hoſpitals, and order that general reports be publiſhed annually; and that ſome mark of approbation be beſtowed upon thoſe ſurgeons, who appear to have executed their duty with the greateſt diligence and ability. It ought to be a concern of government, however, that no perſon be appointed to inſpect regimental hoſpitals, who is not well acquainted with the diſeaſes of the climate, in which the troops happen to ſerve.

NOTES.

CHAP. I.

THOUGH I have described the endemic fever of Jamaica as distinctly as is in my power; yet as I have often observed that we attain more accurate ideas from the detail of a particular history than from general description, I shall select from my notes two or three cases which may serve to give a clearer view of the different species of the disease. And in the first place I shall describe an instance of fever, which was distinguished through the whole of its course by symptoms of the general inflammatory diathesis.

(1) Lennox, a soldier of the 60th regiment, aged 40, of a firm and compact habit of body, was seized on the 3d of December, between eight and nine in the morning, with a slight horror or shivering, preceded and accompanied by other usual marks of fever. The symptoms of coldness and shivering went off in the course of eight or ten minutes. A hot fit succeeded, with a good deal of head-ach, hurried respiration, considerable thirst, a strong, full, and frequent pulse. After a continuance of four or five hours, sweat began to appear on the head and breast, which extending gradually to the extremities brought with it a tolerable distinct remission of the fever. 2. The fever appeared to be gone off very completely by ten o'clock at night. The patient rested well during

the night, and continued in this ſame ſtate of eaſe till about five o'clock in the evening. He then became uneaſy and reſtleſs, with head-ach and a ſlight feveriſh heat. 5. The feveriſh indiſpoſition declined in the courſe of the night. He became eaſier towards morning, and about ſeven might be ſaid to be in a ſtate of remiſſion. About nine a paroxyſm commenced, ſimilar to the paroxyſm of the firſt day, though with a ſtill ſlighter degree of horror and ſhivering; the hot fit ran ſtill higher, with much head-ach, thirſt, and a ſtrong vigorous pulſe. The ſweating at laſt made its appearance, and the violence of the fever declined: there ſtill however remained ſome degree of head-ach, pain of the back, and thirſt, with an averſion to food, and a more than natural frequency of pulſe. 6. Reſted tolerably; but ſtill is not free from head-ach and pain of the back: the tongue is dry and foul, and the coat which covers it is ſmooth, but of conſiderable thickneſs, and of a cream colour. About four in the evening the paroxyſm of a fever made its appearance, ſimilar, in ſome degree, to the paroxyſm of the fourth, but of a much greater degree of violence. It continued for eight hours, and declined gradually towards morning. 7. There were no perceivable marks of fever at ſeven in the morning. A little after nine, however, a paroxyſm commenced, ſimilar, in every reſpect, to the paroxyſm of the fifth. 8. About three in the afternoon a paroxyſm began ſimilar to the paroxyſm of the ſixth, but ſtill more violent. It declined after the uſual duration, and was ſucceeded on the ninth by another paroxyſm ſimilar to that of the ſeventh. The remiſſion which ſucceeded appeared to be ſtill more perfect than any of the preceding; the ſweat was even more copious, and the pulſe became ſofter and more expanded after it than it had hitherto done. 10. A paroxyſm returned about half paſt two, ſimilar to the paroxyſm of the eighth, but not leſs violent in degree. It termi-

nated, however, in a more fluid and univerſal ſweat; the pulſe and the ſtate of the ſkin returned perfectly to what they were in health; the mucous coat ſeparated from the tongue; the eye and countenance aſſumed their natural ſerenity, and unequivocal marks of final criſis appeared on the morning of the eleventh. The above caſe is an inſtance of the double tertian; the fever of the even day terminated the diſeaſe; and the pulſe through the whole courſe was vigorous and ſtrong, or marks of inflammatory diatheſis, in a moderate degree, were conſtantly preſent.

(2) Henley, a ſoldier of the 60th regiment, was ſeized on the 6th of May, about five in the evening, with a nauſea, or unpleaſant affection at ſtomach, marks of great languor and debility, a ſlight feeling of coldneſs and horror, a very weak and frequent pulſe, head-ach, pain of the back, and other ſymptoms which are uſual in the acceſſion of fevers. After a continuance of ten or twelve hours, theſe ſymptoms were ſo far gone off, that the patient was conſidered to be in a ſtate of remiſſion. 7. The exacerbation of the fever returned again about the ſame hour in the evening at which it had firſt come on, though without marks of preceding coldneſs or ſhivering. The pulſe was ſmall, obſcure, and extremely frequent; the heat of the body was not increaſed very materially; the thirſt was only in a moderate degree, but there was much nauſea, an averſion to food, a diſpoſition to faint in an erect poſture, deep and heavy ſighing, tremor of the tongue, and a ſad and deſponding ſtate of the eye and countenance. 8. The ſymptoms of fever abated towards morning, and a remiſſion, though by no means a diſtinct one, took place. The pulſe became ſomewhat ſlower and more expanded; the ſighing and anxiety abated a little, and there was evidently a ſtate of greater eaſe; though there ſtill remained marks of great debility, and ſigns

of ſpaſmodic ſtricture on the ſurface of the body. The heat was lower than it uſually is in health. About five in the evening the ſymptoms, which had prevailed in the former paroxyſms, returned again, but with conſiderable aggravation. The head was affected with delirium, and there was a conſiderable degree of tremor and ſtarting. 9. Eaſier in the morning, though the remiſſion was in no degree more complete than the former. About the uſual hour in the evening the ſame ſymptoms returned with aggravation. 10. The remiſſion as the former; the heat of the body below natural; the pulſe obſcure and frequent; the ſigns of debility very great. The exacerbation returned again at the uſual hour; the paroxyſm appeared to be ſomewhat more violent; the delirium was higher, the heat greater, and the pulſe acquired rather more ſtrength and fulneſs. 11. Eaſier in the morning, with a remiſſion in every reſpect as complete as the former; the pulſe diſtinct, and rather more expanded; and, upon the whole, an appearance of rather more vigour. The paroxyſm was renewed in the evening as uſual. 12. Remiſſion in the morning rather more complete: more vigour in the pulſe. The exacerbation as uſual. 13. In the morning, inſtead of the uſual remiſſion, there appeared marks of a complete and final criſis; the ſighing, which had been troubleſome throughout the courſe of the diſeaſe, vaniſhed; the eye and countenance aſſumed their uſual ſerenity and cheerfulneſs; the pulſe became ſlower, ſofter, and more expanded; and the tongue parted with its coat or covering. The above is an inſtance of fever with ſymptoms of nervous affection.

(3) Sergeant Negli, on the 2d of November, about eight in the morning, was ſeized with horror, ſhivering, and other Symptoms, which are uſual in the acceſſion of fevers. The hot fit did not run very

high, and before evening the paroxyſm was conſiderably abated. 3. This patient is now in the ſtate of remiſſion, the heat of the body is not greater than natural; but the caſe ſeems to be attended with ſome ſymptoms which are not very common in the fevers of this country. The countenance is clouded, dark, and grim; the appearance of the eye is ſad and deſponding; and he expreſſes an uneaſineſs in his feelings which is not eaſily accounted for. 4. The paroxyſm returned about four in the morning. It was greatly more violent than the preceding; and though it might be ſaid to remit very completely, if we judge by the heat of the body and ſtate of the pulſe; yet there ſtill remained ſome uncommon and unpleaſant feelings. The eye and countenance were not only dark and deſponding, but the tongue was covered with a ſlimy mucous coat, through which the red ſurface appeared obſcurely; there were ſtrange and unaccountable twitchings of the ſtomach and bowels, diſturbed ſleep, frightful dreams, and foreboding apprehenſions. 5. A paroxyſm came on this evening near twelve hours ſooner than it was expected. After expreſſing an eaſineſs at ſtomach, and throwing up ſome matter of a dark colour, he was ſuddenly ſeized with a ſtupor and inſenſibility, from which he could not be rouſed by all the applications of art. He died in about ſixteen hours. This caſe affords an inſtance of fever with marks of a peculiar malignity. The appearances of danger were ſudden and unexpected; and, as it was among the firſt inſtances of the kind which I had ſeen, I was diſappointed, and in ſome degree confounded at the event.

(4) Thomſon a young man aged twenty, after more than uſual exerciſe in the heat of the ſun, was ſeized with ſickneſs, ſhiverings, and other ſigns of fever, about nine o'clock in the morning of the 3d of February. The pulſe was hard, frequent, and

irritated; the eye was ſad, and ſometimes gliſtening; the countenance fluſhed, but rather dark and overcaſt; the reſpiration hurried; nauſea was troubleſome, with a good deal of anxiety and reſtleſſneſs. The paroxyſm continued long, and did not indeed go off very perfectly at laſt. 4. Reſted but indifferently; is now ſomewhat eaſier, though the remiſſion is far from being perfect; the thirſt is conſiderable; the tongue dry and foul, the ſtomach loathes all ſorts of food; and he ſeems to be much diſtreſſed with flatus and ructus; the ſtools are dark-coloured and fœtid; the pulſe is more frequent than natural, hard and irritated, and the ſkin is only partially moiſt. 5. An exacerbation of fever happened about nine in the morning. The ſymptoms were of the ſame kind as in the firſt paroxyſm, only ſomewhat more violent in degree. The anxiety at ſtomach was particularly diſtreſſing, and there appeared ſtill more evident marks of putreſcent tendency in the alimentary canal. 6. An uneaſy night: an imperfect and obſcure remiſſion: the gums redder than they naturally are: the eye has a gliſtening appearance, and the countenance is ſtill confuſed and clouded: the tongue is dry; the thirſt great; and ructus and flatus are very diſtreſſing: the pulſe ſtill irritated and quick: there is not any very remarkable diſpoſition to faint in an erect poſture: the ſtools fœtid. 7. The exacerbation returned about the ſame hour as on the fifth, and with ſtill greater aggravation: the ſymptoms of diſtreſs in the ſtomach and bowels were particularly alarming; with nauſea, nidoroſe belchings, and large watery fœtid ſtools. 8. Somewhat eaſier in the morning, though the remiſſion can only be ſaid to be obſcure. 9. The exacerbation happened at the ſame hour as on the ſeventh, and continued for nearly the ſame length of time. 10. Inſtead of obſcure remiſſion, marks of final criſis are now evident; the pulſe is returned nearly to its natural ſtate; the eye and

countenance have assumed their usual serenity; the skin is moist, and gives no marks of remaining spasmodic stricture; the anxiety and ructus have ceased; and the state of the stomach and bowels appears to be almost natural. The above is an instance of fever, in which there were very evident signs of putrescent tendency in the alimentary canal; even some obscure marks of its progress in the general system, complicated, however, with an irritated state of the vascular system, or such symptoms as might be considered as belonging to the apparent inflammatory diathesis.

(6) Cunningham, a sailor, aged twenty-five, was seized on the 5th of July, about five in the evening, with sickness, shiverings, head-ach, and the other usual signs of the remitting fever of the country. Its more distressing symptoms were nausea and vomiting. 6. The remission is tolerably distinct; but there is still a good deal of head-ach, thirst, and signs of debility; the tongue is dry, and the pulse is more frequent than natural. The paroxysm returned about five in the evening with increased violence, accompanied with severe retching, and copious vomiting of bilious matter. 7. Better in the morning; the vomiting has ceased, and the remission is tolerably distinct. The exacerbation returned at the usual hour, with the same distinguishing symptoms of copious bilious discharges. 8. Remission in the morning as usual; the exacerbation in the evening as the preceding, with distressing and severe vomiting. 9. The usual remission in the morning. The paroxysm likewise recurred in the evening about the usual time, but not with the usual symptoms. Instead of vomiting of bilious matters, there was some degree of delirium, tremors, startings, and other symptoms of nervous affection. 10. These symptoms remitted in the morning, but there still

remained ſigns of great irritability and weakneſs. The ſame train of ſymptoms returned again in the evening: the delirium and tremors were ſtill in a higher degree; the pulſe was ſmall and frequent; and there was occaſionally a great diſpoſition to faint in an erect poſture. 11. Better in the morning, though there are not yet any marks of criſis. The exacerbation returned again in the evening, with ſymptoms ſimilar to thoſe of the preceding paroxyſm. 12. Remiſſion in the morning ſimilar to the former. Exacerbation in the evening rather more violent. 13. Remiſſion as the former; the pulſe however appears to be rather fuller than it has been ſince this change happened in the circumſtances of the diſeaſe. The paroxyſm returned at the uſual hour ſtill more violent, though with greater marks of vaſcular excitement. 14. Evident marks of criſis: the tongue begins to part with its covering; the eye and countenance appear clear and animated; the pulſe is ſlower and fuller; and the ſtate of the ſkin does not give any indication of exiſting ſpaſmodic ſtricture. This caſe preſents an inſtance of fever, the firſt part of the courſe of which was diſtinguiſhed by uncommon bilious diſcharges during the time of the paroxyſms; the latter part of it by affection of the nervous ſyſtem.

CHAP. II.

1. As I mentioned before that we attain more accurate ideas from the detail of particular cases than from general histories; I therefore relate the method of cure, which was pursued in those examples which are described in the sixth chapter.

1. Lennox.—On the 4th of December, or second day of the disease, the solution of salts with a small portion of emetic tartar was given by a wine glass full at a time, till it operated plentifully. 5. Some powders of nitre and camphire. 6. Two scruples of bark were given every two hours during the remission, with an injunction that the nitrous powders be repeated during the time of the paroxysm. 11. The above plan was persisted in till marks of crisis appeared. Not more than one ounce of bark was given during all the remissions.

2. *Henley.* 7. The usual solution of salts was given, but without any addition of emetic tartar. It operated plentifully. 8. The bark was begun this morning, with injunctions that it be administered every two hours during the remissions. 9. A blister was applied to the back of the head and neck, with a bolus of camphire, opium, and valerian. Wine was ordered, together with the bark, as soon as the remission should begin to appear. This plan was persisted in till the crisis arrived, which was on the 13th.

3. *Negli.* The patient was purged on the 3d with the usual solution of salts, to which was added so considerable a portion of emetic tartar, that it likewise operated by vomit. 4. Bark was given in the

uſual quantity, and at the uſual intervals. 5. As ſoon as the fever came on, bliſters were applied to the head, and likewiſe to the legs; but they produced no perceivable effect. The patient died, and probably fell a ſacrifice to the diſeaſe, from my not having early enough perceived the malignity of its nature.

4. *Thompſon.* 4th, The ſolution of ſalts with emetic tartar was adminiſtered in the preſent caſe as it had been done in the others. It operated plentifully, but had no material effect upon the diſeaſe. 5. Saline draughts in the ſtate of effervescence were given frequently. Bark and wine were ordered in the remiſſions, with as much lemonade as the patient choſe to drink. 9. Glyſters of cold water, impregnated with fixed air, were employed two or three times with apparent benefit. 10. The bark, wine, and ſaline draughts were given liberally, yet notwithſtanding, the diſeaſe ſeemed to complete its natural courſe.

5. Cunningham.—6. The nauſea and vomiting were ſo diſtreſſing in the firſt paroxyſm, that, in compliance with the patient's earneſt entreaties, I conſented to give an emetic. 7. The ſymptoms were aggravated, and the emetic was repeated but without advantage. 9. Anodynes were given during the paroxyſm with ſaline draughts in the act of effervescence. They moderated the vomiting but did not entirely remove it. Bliſters were applied to the head and legs; bark and wine were given in conſiderable quantity; but the diſeaſe continued till the 14th without material alteration.

CHAP. III.

1. As the cold bathing, which I have ſo ſtrongly recommended in the cure of fevers, has an exterior appearance of being a raſh and hazardous remedy, I ſhall relate ſome caſes which may enable the reader to judge more preciſely of its real effects. Cold-bathing I may remark, appears to have been occaſionally employed by the Greek and Roman phyſicians, after the time of the Emperor Auguſtus; but I was only a young man when I went out to the Weſt-Indies, and cannot pretend to ſay that I was acquainted with the writings of thoſe phyſicians, or that I poſſeſſed much knowledge of diſeaſes, except the little that could be retained from a curſory hearing of univerſity lectures. The firſt hints of this practice were therefore accidental, and aroſe from a converſation I had with the maſter of the veſſel, in which I went paſſenger. This perſon commanded a tranſport in the war 1756, and was preſent at the ſiege of the Havannah. As he was talking one day of the ſtate of the fleet, he mentioned accidently, that ſome men were ſent aboard of his ſhip ill of fevers; ſeveral of whom, he obſerved, jumped into the ſea during the dilirium which attended the paroxyſms of the diſeaſe. Some of them, as might be expected, were drowned; but the moſt part of thoſe who were recovered from the waves appeared to be greatly benefited by the ducking. The fact, which, from the veracity of the man, I thought I could depend upon, ſtruck me ſtrongly, and I reſolved, in my own mind, to bring it to the teſt of experiment as ſoon as an opportunity ſhould offer: neither was it long after my arrival in Jamaica, that I had occaſion to viſit a ſailor whoſe ſituation ſeemed to juſtify ſuch a trial. The poor

man was aboard of a ſhip, which lay at anchor about a mile from the ſhore. He had been ill two days; the delirium ran high; his eyes were red and inflamed; his reſpiration was hurried; he was anxious and reſtleſs in a high degree, whilſt together with thoſe marks of excitement, he was occaſionally languid and diſpoſed to faint. His ſkin being dirty furniſhed an oſtenſible excuſe for trying this remedy. But it was previouſly thought proper to draw ſome blood from the arm; which being done, ſome buckets of ſalt water were daſhed on the ſhoulders. He was now laid in bed: a copious ſweat enſued, ſucceeded by a diſtinct remiſſion, and a total change in the nature of the ſymptoms. The ſucceſs I met with in this inſtance was more than I had expected; I was therefore encouraged to try the ſame mode of bathing in a perſon who came under my care ſome weeks thereafter, and who had been ill of a fever ſix or ſeven days. This patient had been bled and bliſtered;—emetics and cathartics had been likewiſe employed, and bark had been given in the uſual manner, for the three laſt days. The fever, however, had now in a great meaſure loſt its type. The man was low and languid; his eyes were dim; his viſion indiſtinct;—his pulſe was ſmall and frequent, and, when the head was raiſed from the pillow, not to be felt. Though it did not appear that he could reaſonably be expected to live long, I ſtill wiſhed to get him conveyed to the deck, that a trial might be made of the effects of cold bathing; but the ſituation was ſo tickliſh, that I felt ſome uneaſineſs in ſetting about it. At laſt he was lifted through the hatch-way in a blanket, though I muſt confeſs that I was not without apprehenſions that he might die under my hands. Some wine was then poured down his throat; and he was ſprinkled with cold ſalt water, as he lay on the deck. Appearing to be ſomewhat invigorated by this proceſs, he was raiſed up very gently, and ſeveral buckets of

the sea-water were dashed about his head and shoulders. He was then laid in bed; the pulse soon became large and full. I left him in a copious sweat, and was agreeably surprised next day to find him sitting on the deck, to which he had walked on his own feet. I shall only mention another instance of the good effects of cold bathing in the fevers of the West-Indies, which is perhaps more decisive than either of the former. A boy, aged fourteen, had been ill of a fever seven or eight days. Nothing had been omitted, in point of treatment, which is usual to be done in similar cases. Bark and wine had been carried as far as could be serviceable, or even safe; yet death seemed to be approaching fast. The success of cold bathing, in some instances similar to the present, so far exceeded my expectation that I was induced to make trial of it in the case before me, though I was not altogether without apprehensions that death might be the consequence of the attempt. The business, however, was accomplished without accident; and next day the boy was able, not only to sit up in bed, but even to walk over the floor. After instances so unequivocal as the above, it would be superfluous to mention any others. I shall only add, that I have tried the remedy, in various situations, always with safety, generally with astonishing success; so that I cannot forbear recommending it even at an early period, in the fevers of the West-Indies. It communicates tone and vigour to the powers of life, and diminishes irritability in a degree far superior to all other cordials or sedatives. The bathing was managed in the following manner: the water, which was required to be of a refreshing degree of coolness, was generally dashed by means of a bucket on the head and shoulders. It was likewise found that its good effects were heightened, in some cases, by previous bleeding, and by the previous use of warm bathing. This may seem a rash practice to those who argue

without experience; but, ſetting aſide the authority of the ancients, we find it confirmed by the example of a perſon who was not a phyſician, and who, therefore may be ſuppoſed to be leſs under the influence of a favourite opinion from which he might be led to diſguiſe the truth. Buſbequius, who was ſent on an embaſſy to Soliman the Great, was obliged to travel to Amaſia, where the Sultan then ſojourned. In his return home he was ſeized with a continued fever, and very ſeverely harraſſed by it. The diſeaſe gained ſo much ground during the journey, that he found it neceſſary to ſtop at Conſtantinople to attend to the recovery of his health. The practice which was adopted to effect this may appear to be ſingular, and by many, perhaps, will be thought to be hazardous and raſh. He mentions, that, after enjoying the luxury of warm bathing, he was ſuddenly ſprinkled with cold water. His words are, "Idem, ſcilicet, Quaquelbenus me a balneo exeuntem frigida perfundebat, quæ res etſi erat moleſta, tamen magnopere juvabat." (Iter. Conſtant. p.) His phyſician, Quaquelbenus, who ſeems to have been a man of excellent judgment and careful obſervation, had probably learnt the practice in his travels in Aſia, as it does not appear to have been commonly known in Europe at that time.

CHAP. IV.

1.—But beſides theſe teſtimonies of phyſicians, in favour of the practice of drenching with cold water, the memoirs of Baron Trenck furniſh us with a curious and very convincing proof of the efficacy of this remedy, in extinguiſhing, almoſt like a charm, the violence of a burning fever. The Baron, when ill of a fever in the priſon at Madgeborough, unfortunately broke the pitcher which contained his daily allowance of water. The fever was violent, and he ſuffered the moſt inexpreſſible torments of thirſt, for the ſpace of twenty-four hours. When the uſual ſupply was brought to him next day, he ſeized the pitcher with eagerneſs, and drank the water with ſuch avidity, and in ſo great quantity as is ſcarcely credible. The conſequence was a total removal of the diſeaſe. To this I might add an inſtance, which happened to myſelf at Savanna in Georgia, in the year 1779. In the exceſſive hot weather of the month of July, I was ſeized with the endemic of the country, in a more violent degree than was commonly ſeen. In the third paroxyſm of the diſeaſe, my deſire for cold water was ravenous. A pitcher of it was drawn from the pump, which I drank off inſtantly without the leaſt abatement of the thirſt. The draught was repeated in a few minutes, in quantity not leſs than a quart. The thirſt was effectually quenched, and the fever ſeemed to vaniſh. But though the fever appeared to be extinguiſhed as it were by a charm; yet the ſtomach and hypochondria became diſtended, yellowneſs of the eye and countenance ſucceeded, with a conſiderable degree of debility which remained for two or three days. I muſt, however, remark with regard to this caſe, that the effects were not the ſame as they have been uſually reported to be by

authors. The fever was extinguiſhed; but neither vomiting, ſweat, or any other ſenſible evacuation enſued. The ancients, I may further obſerve, ſeem to have adminiſtered cold drink only in the advanced ſtate of fever, when ſigns of coction began to appear; in which caſe, it is impoſſible to form a certain opinion of its preciſe ſucceſs. That cold water may be employed with effect, it is neceſſary that the thirſt be intenſe, perhaps that it be purpoſely provoked, and that it be fully and completely ſatiated. If managed in this manner, it probably will not often fail of extinguiſhing the fever; yet I muſt not omit to mention, that unleſs it is managed with a great deal of caution and judgment, it may alſo often irrecoverably extinguiſh the powers of life.

2. In ſupport of this opinion, I ſhall mention a caſe, which fell under my own obſervation about a twelvemonth ago. I was called to a young man, a ſailor, ill of a fever of a very dangerous and alarming kind. It was the eighth day of the diſeaſe before I ſaw him. He had not been hitherto in the leaſt benefited by any thing that was tried; neither did any remedy which I could think of, though employed with almoſt deſperate boldneſs, in any degree check the progreſs of the diſeaſe. The power of ſpeech was loſt, and even ſwallowing was performed with difficulty; the eye was languid, nay almoſt without motion; the countenance was ghaſtly; and many livid ſpots, ſome of them nearly the ſize of a ſix-pence, made their appearance on different parts of the body. I propoſed bathing, and the friends of the young man, conſidering the ſituation as deſperate, conſented that I ſhould make a trial of it; more, perhaps, to comply with my deſire, than from expectation of any benefit that might reſult from it. But in ſetting about it, it unfortunately happened, that an utenſil proper for the purpoſe could not be procured, ſo that

we were compelled to be contented with a general fomentation. This was applied in as complete a manner as circumſtances would permit. A blanket was ſoaked in warm ſalt water, and the body was wrapped in it from head to foot. In a ſhort time the ſkin became ſoft and warm, ſweat began to flow; the eye and countenance began to reſume their animation, which had been almoſt extinguiſhed, the pulſe roſe; ſwallowing was performed with leſs difficulty; and next day the colour of the ſpots was evidently brighter. So far the change was favourable; but a regular ſupply of wine and cordials having been neglected during the following night, the pulſe ſunk, and things returned nearly to their former ſituation. The fomentation was again repeated, in conſequence of which the extremities and ſurface of the ſkin became warm and moiſt, an effect which was no ſooner produced, than the blanket was ſuddenly removed, and the face and breaſt, particularly, were ſprinkled with cold water, in which a large portion of ſalt was diſſolved. The cold had the effect to cauſe the patient to ſhrink at the firſt, yet in a ſhort time he appeared to be refreſhed very remarkably. The powers of life grew gradually ſtronger; though the marks of criſis were not very evident for ſeveral days. To the above I might add ſome other inſtances, where effects were ſimilar; but I avoid ſwelling the notes to too great extent, by entering into particular details. I ſhall therefore only obſerve in general, that cold bathing was uſually of ſervice. It imparted general tone and vigour to the powers of life, and by increaſing the activity of the vaſcular ſyſtem, probably ſometimes rendered the criſis complete, where it naturally would not have been ſo; but I cannot venture to ſay that I ever carried it ſo far that the diſeaſe could be ſaid to be precipitately extinguiſhed by it.

NOTES

TO THE

CHAPTER UPON YELLOW FEVER.

(a) IN compliance with the language of medical authors, I have described the following disease under the name of Yellow Fever, though I am perfectly sensible, that the appellation is not by any means proper. There are some instances of the disease perhaps where yellowness does not at all appear, and in no one does it ordinarily shew itself till the latter stages. I know also that most of the practitioners of Jamaica consider it only as an aggravated species of the remittent; the common endemic of hot climates. It appeared to me I must confess in a different light; but I shall attempt to describe the two diseases accurately, and leave it to the reader to judge for himself. It may not however be improper in this place to take notice of the opinion of Dr. Moseley. Dr. Moseley has lately written a treatise on this disease, and endeavoured to persuade us that it is no other than the Καυσος, or ardent fever of the ancients. But the yellow fever of the West-Indies, by Dr. Moseley's own confession, is in some manner peculiar to strangers newly arrived in tropical climates. The Καυσος we are informed, made its appearance in the islands of the Archipelago, and on the coasts of the contiguous continents indiscriminately among men and women,

natives or foreigners: in fact it has not, as far as I can perceive, any claim to be considered as a distinct species of disease. If I rightly understand the spirit of Hippocrates, or the description of the still more accurate Aretæus, Καυσος in reality is only an accidental condition of the common endemic of the country, where the force of the fever is chiefly exerted upon the stomach and alimentary canal. In this manner it appears frequently in Jamaica, and in the southern provinces of America. In the hot months of summer, it appears occasionally in every climate: and is not necessarily accompanied with, nor does it depend upon a general inflammatory diathesis of the system for its existence.

6. Authors seem generally to have attributed the black colour of the vomitings observed in this disease to blood effused into the cavity of the stomach; but the falsity of this opinion is sufficiently proved by the appearances which are observed on dissection.

THE END.

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